Metal Progress

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RYERSON STEEL

Strength and Ductility

Extracts From Twentieth Campbell Memorial Lecture

By Maxwell Gensamer
Professor of Metallurgy and Head, Department of Mineral Technology
School of Mineral Industries, Pennsylvania State College

I PROPOSE to discuss "mechanical metallurgy", the relationships among the measured mechanical properties of metals and their mechanical behavior in service, and how they are controlled by chemical composition and structure. For this I will concentrate on the problem of how alloying elements in solid solution and undissolved compounds distributed in these solid solutions affect, quantitatively, the mechanical properties of steels.

Broadly there are two aspects to mechanical properties, namely strength and ductility. By strength we mean the resistance of a substance to *(a)* flow or to *(b)* fracture, and by ductility we mean the amount of deformation prior to fracture. Here also there are two distinct aspects to the problem. First, we must know what capacity a metal has for deformation locally, and second, we must know how the deformation distributes itself.

[The lecturer mentioned numerous variables that influenced strength, the difficulties of determining more than a single point on the curve showing resistance to fracture versus strain, and the possible errors in assuming that any single longitudinal stress plotted in the curve of resistance to flow versus strain will represent exactly the three principal stresses actually existing at any point.]

Our discussion of flow strength is to be based on the stress-strain curve obtained in tension. The data for a typical iron alloy may be plotted in various ways, for example, stress against strain, load against strain, load against elongation, and so on. It is our experience that, over the whole of the stress-strain curve in which we may have confidence, the curve is always a straight line when log stress is plotted against log strain. (See Fig. 1, page 732.)

This straight line means that tensile stress-strain curves (Cartesian coordinates) are parabolas of the equation

$$\sigma = \sigma_1 \delta^n$$

where σ is the true stress
σ_1 is the true stress at unit strain (that is, where $\delta = 1.0$)
δ is the true strain ($\ln l/l_o$)
and n is the "strain hardening exponent" (and the slope of the log-log plot)

The constants which describe completely the whole course of the corrected stress-strain curve are σ_1 and n.

We have used not σ_1 but $\sigma_{0.2}$, the stress for a strain of 20%, because the only reliable points in a stress-strain curve for steel lie between $\delta = 0.1$ and 0.3. Below $\delta = 0.1$, the points are quite reliable if the specimen is aligned very carefully before starting the test, and if a correction is made for the elastic strain. But above $\delta = 0.3$, necking introduces uncertainties. Extrapolation from $\delta = 0.3$ to $\delta = 1.0$ can get you into trouble. On the other hand, the stress at $\delta = 0.2$ is extremely reliable, and independent of the slope n of the line drawn through the points. The equation for the curve can just as well be written

$$\log \sigma - \log \sigma_{0.2} = n\,(\log \sigma - \log 0.2)$$

which is $$\sigma = \sigma_{0.2}\left(\frac{\delta}{0.2}\right)^n$$

Observe that the stress at the maximum load (which would be the tensile strength if the original area had been used instead of the actual area) is a point of no particular significance, for it does not come at a particular value of strain. The stress at $\delta = 0.2$ is the stress at the strain which is about average for the strain at maximum load, in iron about 20%, but varying from 10 to 30%. I shall call this stress at 0.2 strain the "flow strength", in the same way that we have been call-

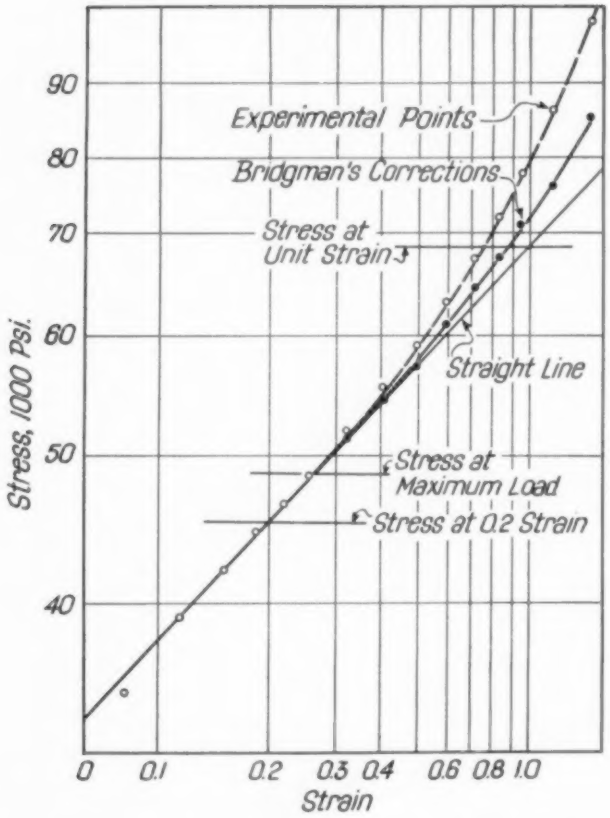

Fig. 1—Double Logarithmic Plot of Stress Versus Strain in Tension for a Typical Iron Alloy, Showing How Bridgman's Corrections for Lateral Stresses Bring the Experimental Positions of Points Near Ultimate Strength Close to the Theoretical Straight Line

ing the stress at a strain of 0.2% (or 0.002) the "yield strength". Flow strength is at 20% strain; yield strength is at 0.2% strain.

[After a discussion of the relationship between simple tensile curves and curves for biaxial stretching, compression, and torsion, the lecturer proceeded to analyze the effect of composition and structure on the tensile data.]

For a wide variety of steels there is a fairly definite relationship between the flow strength and *n*, the strain hardening exponent (Fig. 2). Whether the scatter of points is experimental or real and a result of variations in the alloys remains to be seen. It seems likely that we may expect variations of *n* at a given strength level; a less variation at high strength levels, a larger variation at low. As a first approximation, all stress-strain curves for steel belong to the same family; as the strength level increases, the strain hardening exponent and therefore the rate of strain hardening diminishes.

Now to tell you where we stand on what we started after.

First, the effects of alloying elements dissolved in carbon-free ferrite. Figure 3 contains some previously unreported results, and all the data have been reworked until the plots are thought to be free of error caused by residual alloying elements.

I should like to call your attention to the courage that we have displayed in drawing the line as we have between the two plotted points for phosphorus; most investigations would have drawn a line of quite different slope, in fact negative rather than positive. But we have the courage of our convictions; we refuse to believe that the way to soften steel is to add phosphorus! A straight line of a given slope through a single point, as for beryllium, is also a good trick. And maybe all the copper in this high copper alloy we studied is not in solution, or the solution is supersaturated; if so, copper is not as effective as we think it is.

We have found that the effects of dissolved alloying elements are additive in a simple way. To calculate the strength to be expected in carbon-free alloys containing two or more alloying ele-

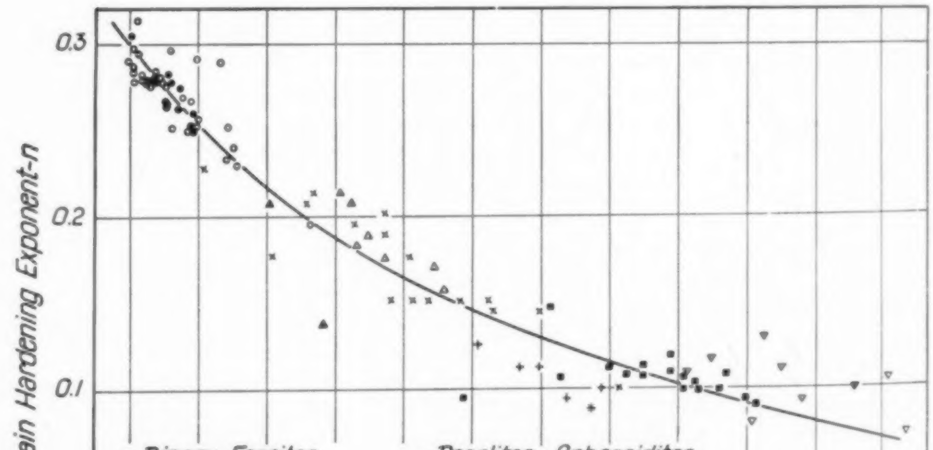

Fig. 2—Strain Hardening Exponent Versus Flow Strength of a Wide Variety of Steels in Various Structural Conditions. If the horizontal coordinates were plotted logarithmically the averaging curve becomes a straight line

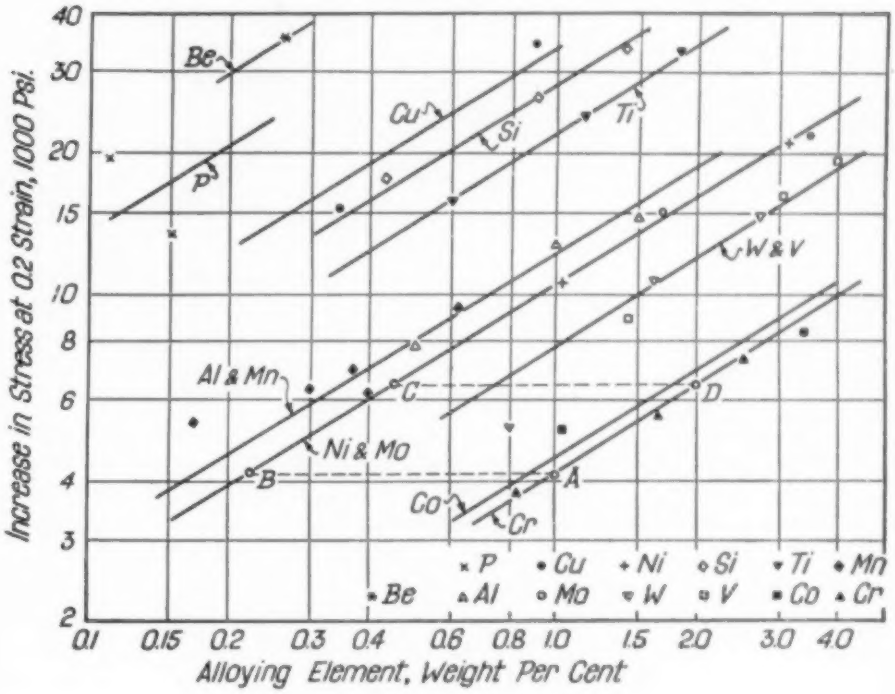

Fig. 3 — Strengthening Effect of Alloys in Solution in Carbon-Free Ferrite — the "Solid Solution Effect". (Flow strength of unalloyed ferrite is 30,000 psi.)

ments, from data obtained with simple binary alloys, we have used the concept of equivalent nickel concentration. Nickel is soluble over a wide range of concentrations and has a moderate effect on strength. To obtain the nickel equivalent of another element, simply observe from Fig. 3 what concentrations of nickel will increase the strength the same amount as 1% of the alloying element in question. For example, 1% of chromium (point *A*) is equivalent to 0.22% of nickel (point *B*). Then, to express the concentration of chromium in an alloy as the equivalent nickel concentration, multiply the amount of chromium present by 0.22; for a 2% chromium steel this would be 0.44% equivalent nickel. The same result is obtained by reading across from the nickel line in Fig. 3 to the chromium line; 0.44% nickel (point *C*) strengthens iron by the same amount as 2% chromium (point *D*).

One can also make a pretty good estimate of the strength of ternary or higher iron alloys by adding the equivalent nickel concentrations of all the alloying elements present, and looking up the strength on the nickel line of Fig. 3.

[After a brief comment on the correlation — more or less good — between strengthening effect of solid solution and other properties, such as limit of solubility, lattice distortion, electrode potential, atomic number, the lecturer proceeded.]

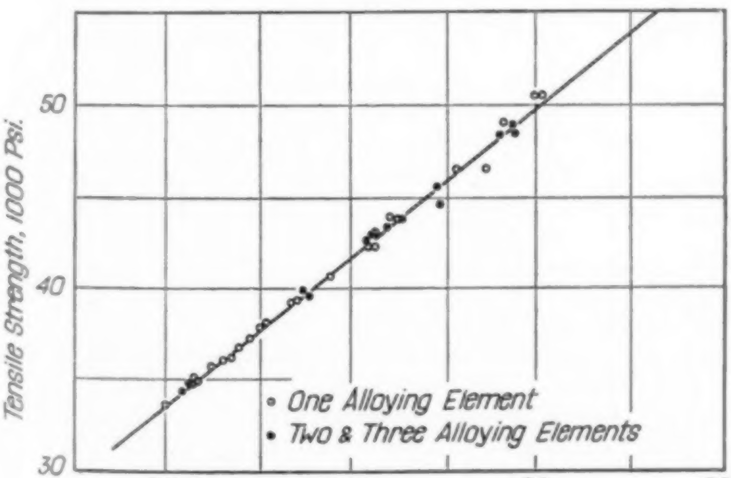

Fig. 4 — Relation Between Tensile Strength and Flow Strength of Alloyed Ferrite (Carbon Free)

If you should be more interested in tensile strength than in stress at 0.2 strain (flow strength), Fig. 4 shows how regular is the relationship. For a complex solid solution alloy, the tensile strength may be calculated from the total equivalent nickel concentration, looking up the flow strength from Fig. 3, and then getting the corresponding tensile strength from Fig. 4.

Now for the strengthening effects of hard particles dispersed in iron: All the steels used in this study had almost the same alloy content and, from what we have just reported, we would expect any effects caused by minor variations in alloy content to be quite negligible. Figure 5 is for the stress at 0.2 strain and shows the dependence of flow strength on the mean distance from one carbide particle to another. The pearlites, both eutectoid and hypo-eutectoid (0.78% carbon and 0.38% carbon, respectively) both seem to plot a little above the averaging line; the spheroidites (quenched and tempered) on or below it. (The line is drawn to minimize the squares of the deviations.) The pearlite spacing measurements were quite tricky and are not wholly to be relied upon. The spheroidite measurements are straightforward — you simply count the particles touching a number of lines across the photomicrograph, but this method permits only the measurement of relatively coarse structures. For the hypo-eutectoid steels, the ferrite path is the average for the whole alloy, across both ferrite and pearlite areas. Strength is not sensitive to local variations in structure; the alloy averages its strength over considerable volumes.

Observe that the amount of strengthening to be obtained in even coarse dispersions (Fig. 5) outweighs the strengthening effect of even large concentrations of alloying elements dissolved in ferrite (Fig. 3). While it would be interesting to see how dispersion hardening and solution hardening add up when both effects are used in the same alloy, it is quite impractical to make the comparisons necessary because solution hardening is relatively so ineffective. The amount of strengthening to be expected by adding alloying elements to a carbon steel of fixed ferrite path would lie altogether within the scatter band of these experiments; going from a plain carbon steel to the highest alloyed of the S.A.E. steels might move the strength from the lower limit of the range to the upper limit. We may safely conclude what you all already know — that alloying elements are useful to control reaction rates and carbide dispersion but have no value for strengtheners as such.

[The second half of the lecture dwelt upon ductility — a very complicated subject, as illustrated by the results of four different tests on sheet metal, *(a)* a narrow strip, *(b)* a very wide sheet, *(c)* unbalanced biaxial tension, or elliptical bulge and *(d)* balanced biaxial tension or spherical bulge, and by the distribution of strain (gage length) in each test. The lecturer pointed out that the uniform strain, remote from the necking down and fracture, is about the same in all these tests, whereas the local ductility at the fracture varies greatly. Likewise it was shown that few generalizations are possible about the factors that affect ductility (strain gradient) — the relation between structure or composition and ductility, for example. Much work has been done on the effect of speed of testing and the effect of temperature on the energy absorbed during plastic deformation, but the lecturer warned that the results should be interpreted with care — the influence of local deformation at a notch may be overshadowed by the importance of the strain gradient. Some mathematical expressions were presented to define and relate the variables.]

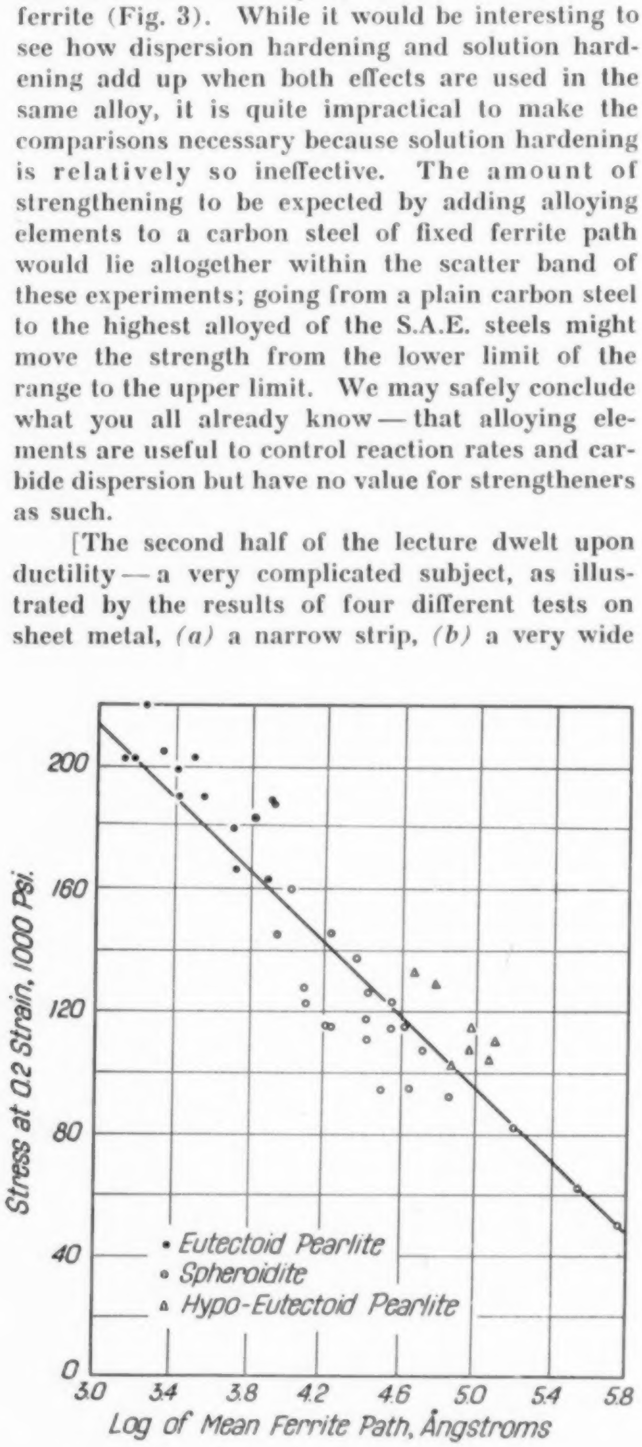

Fig. 5 — Strengthening Effect of Pearlite Plates or Spheroids on Carbon-Free Iron Alloys

Conclusion

I should like to summarize briefly what I have said, for the sake of emphasis.

1. Strength is not sensitive to local variations in composition and structure.
2. In solid solutions it depends in a very regular way on composition.
3. In aggregate structures it depends in a very regular way on the mean spacing between particles and not at all on the shape of the particles except as this affects the mean spacing.
4. The effect of dissolved elements is insignificant in comparison to the strengthening effect of fine dispersions (hardly a new observation).
5. Ductility on the other hand is much affected by local conditions.
6. But local ductility, unless very low, has little effect on energy absorption in a structure; the distribution of deformation is apt to be of more consequence.
7. Efforts to improve energy absorption in structures by improving the local limiting deformation at the point of breaking are liable to be disappointing.
8. Improvement in deformation distribution by modification of the plastic properties of substances should be more profitable, when the intrinsic ductility is considerable.
9. However, this should not be true at high strength levels, where intrinsic ductility is low enough to be a more important factor. It is still important for us to try to improve the limiting, local deformation prior to fracture in high strength materials, even though it be of less importance in our more ductile alloys.

Acid Slags

Slags in Acid Electric Steelmaking

By Conrad C. Wissmann
Construction Battalion, U S. Navy

THERE have been four articles published at intervals in *Metal Progress*, written by the present author, in which he has tried to present an elementary view of the important factors surrounding the melting, working, deoxidation and the final additions to a heat of acid electric steel. In this concluding article the important subject of slags will be considered more specifically than has been done in the preceding articles.

When steel is melted in the acid electric furnace there always forms on the top of the metal some viscous material called slag, made up of oxides of various constituents which were in the metal that is being melted, together with a little silica from the hearth, walls and roof of the furnace. Slag is principally a mixture of oxides — silica (SiO_2), iron oxide (FeO), manganese oxide (MnO), and calcium oxide (CaO). It usually contains some aluminum oxide (Al_2O_3) and magnesium oxide (MgO), but the percentage of these two are very small and therefore can be disregarded in this elementary discussion.

The steel scrap when put into the furnace will contain various amounts of the elements silicon and manganese alloyed with it, depending on the type of scrap which was charged. While the metal is melting — that is, after it has reached a red heat — it has the ability to absorb oxygen from the atmosphere in the furnace. The oxygen principally combines with the iron (because there is far more iron in the charge than any other element) and forms Fe_2O_3 — or as it is commonly called, scale. This scale later comes in contact with molten metal (principally iron) and is converted to FeO, a lower oxide of iron, according to the following equation:

$$Fe_2O_3 + Fe = 3FeO$$

This lower oxide of iron can go into solution with the molten metal and so come into molecular contact with the silicon and manganese already in the bath. Since they have a greater "affinity" for oxygen than has the iron, they tend to react with the FeO and form their own oxides, silica and manganese oxide, respectively, as noted in the following equations:

$$Mn + FeO = MnO + Fe$$
$$Si + 2FeO = SiO_2 + 2Fe$$

These oxides, since they are practically insoluble in molten iron, and have a lighter specific gravity than the metal, will tend to float up to the top of the bath, forming this viscous layer called slag. Also the corrosive action of the metal on the silica sand of the hearth will wash away part of the hearth, and these particles will also tend to rise up through the metal to form slag.

The varying amounts of the different constituents in the slag can be estimated from its color. A slag high in silica and low in iron oxide will have a pea green color. On the other hand, a slag higher in iron oxide and lower in silica than the usual run in acid practice will have a black color. Such a black slag might analyze 35% FeO and 45% SiO_2. The melter therefore uses the color of the slag to give him an indication of its composition. This is always important for it is always necessary for him to have a rough idea as to the amount of iron oxide (FeO) which remains in the metal and, as has been stated before, the amount of FeO in the slag is in proportion to the amount remaining dissolved in the steel bath. While it is melting down and immediately after the meltdown, the bath should have a high FeO content, but when it is to be tapped, the FeO content should be as low as possible.

While in the furnace the slag and metal can both hold FeO. There is a law of chemistry known as the "distribution law of Nernst" which may be stated briefly as follows: "If a substance be dissolved in two liquids which are in contact yet insoluble in each other, that substance is distributed between those two liquids at a given temperature in a constant proportion when equilibrium exists." Mathematically stated in our case it would be:

$$\frac{C_1 \text{ (FeO in Slag)}}{C_2 \text{ (FeO in Metal)}} = K$$

where the concentrations of the FeO in the slag (C_1) and of the FeO in the metal (C_2) represent the molecular concentrations. This equation tells us that, when equilibrium is reached, there is always a definite ratio between the amount of FeO which is in the slag and FeO in the metal. We therefore know that if the slag is black, indicating a high FeO content, the metal will also have a high FeO content. Also, if the slag is a pea green color, indicating a low FeO content, it is always an indication that the FeO content of the metal is also low. The slag in the furnace is therefore a prime source of information to the melter since he can determine from its color the amount of FeO in the metal at any time during the heat, and it is this information in which he is primarily interested during every stage.

Besides the color of the slag there is also an accurate method of determining the FeO content of the slag by measuring its viscosity. The instrument is called a "viscosimeter" and was developed by C. H. Herty, Jr., currently president of the ⊕, during the course of a research into the physical chemistry of basic steelmaking. For the acid slag its design has been modified somewhat. The form recommended by the American Foundrymen's Association is shown in the adjoining sketch, Fig. 11.

Use of Viscosimeter

To use the viscosimeter a sample of the slag is taken in a heated spoon from the top of the bath near the electrodes and poured into the funnel of the instrument. The distance that the slag will run out through the thin hole in the horizontal leg is then measured. This distance will be greater the higher the FeO content of the slag.

However, it should be remembered that the viscosity of the slag, determined in this manner, is not a *direct* measure of its FeO content. Rather, it is a measure of the *acidity* of the slag, and in the slags produced in an acid electric furnace, silica (SiO_2) is the acid constituent. As remarked above, iron oxide (FeO), manganese oxide (MnO) and calcium oxide (CaO) are the principal basic constituents. The "acidity" of the slag is usually thought of as the ratio of the amount of the acid constituent to the total amount of the basic constituents. The more highly acid the slag (the larger the amount of SiO_2 in proportion to the bases) the more viscous it will be; the more basic the less viscous. These statements are true for those mixtures of oxides that correspond with the usual run of acid furnace slags, so the viscosity as measured by the viscosimeter tends to show the degree of acidity of the slag — that is, the higher the per cent of silica. It is of course true that in addition to the iron oxide, the manganese oxide and the calcium oxide content of the slag also contribute to its basicity. Therefore, the higher the amounts of these two constituents in the slag, the less viscous the slag would be. However, the percentages of these two elements in the slag are usually fairly constant for different heats, whereas the FeO content is the major variable. Iron oxide will vary depending on the amount of ore which was added to the heat and the amount of FeO which the metal picked up from scale when it was melting down. Therefore, it can be safely said that the viscosity of the slag is largely dependent on its FeO content.★

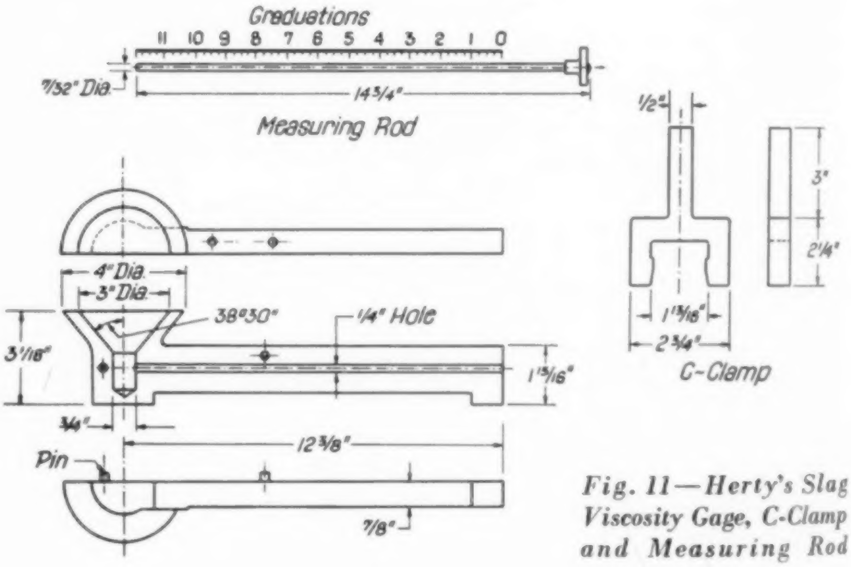

Fig. 11—Herty's Slag Viscosity Gage, C-Clamp and Measuring Rod

The viscosimeter is particularly useful in working out a standard practice — that is, a practice which is adhered to as nearly as possible for every heat. Also it is useful to perfect the skill of

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★More detailed information on this slag viscosimeter is available from an article by John Juppenlatz on "Acid Electric Furnace Slag Control", published as Preprint No. 42-28 by American Foundrymen's Asso., Chicago.

the melters. By use of this viscosimeter a less experienced man can follow the working of a heat. After the meltdown he can test the viscosity of the slag and from its value can look on a chart to determine the amount of ore necessary to put the steel in the proper degree of oxidation. When the metal is hot enough to tap he can take a sample of the slag and test it in the viscosimeter and from its value will know whether the heat is then in condition to be tapped, or whether it should be held a little longer in the furnace to deoxidize further. (By "deoxidize" is meant the removal of the FeO or oxygen from the molten metal.)

Some melters like to add lime (calcium oxide) to their slag. It is usually added as lime rock before the boil or during the boil. The author prefers to add it when the boil is beginning to diminish. It increases the basicity of the slag and thus has the effect of making the slag thinner or less viscous. It also sets free some FeO from the slag to go into the metal since it will displace that portion of the FeO in the slag which was in chemical combination with the SiO_2, according to the following reaction:

$$CaO + SiO_2 \cdot FeO = CaO \cdot SiO_2 + FeO$$

This extra FeO will then react with carbon as soon as it is dissolved in the metal, and this reaction will prolong the boil for a few minutes.

As stated before, this lime addition will also stop a silica reduction. However, lime is primarily added to thin the slag. A slag having little lime in it will hold approximately 10% FeO when it is a pea green color. When the iron content goes lower the metal will start to reduce silica from slag and furnace lining. However, if one or two small shovels of lime rock are added to the slag, the FeO content can be considerably reduced without danger of getting into a silica reduction. This also reduces the amount of FeO which the steel can pick up from the slag after the deoxidizers have been added — a factor that is judged most important by many expert steelmakers. It must be remembered that a lime addition, to have the good effects listed, must be made when there is still an excess of carbon in the bath to act as a reagent to reduce the FeO as soon as it leaves the slag and enters the molten metal. A lime addition would otherwise put oxide into the refined metal where it certainly is not wanted.

Nominating Committees

IN ACCORDANCE with the Constitution of the American Society for Metals, President C. H. Herty, Jr., has selected a nominating committee for the nomination of president (for one year), vice-president (for one year), and two trustees (for two years each). This committee was selected by President Herty from the list of candidates submitted by the chapters. The personnel is:

Glenn Coley, Detroit Edison Co. (Detroit Chapter); chairman.

C. D. d'Amico, Joshua Hendy Iron Works (Golden Gate Chapter).

R. S. Lynch, Atlantic Steel Co. (Georgia Chapter).

J. E. Drapeau, Jr., Metals Refining Co. (Calumet Chapter).

D. F. McFarland, Pennsylvania State College (Penn State Chapter).

O. W. Ellis, Ontario Research Foundation (Ontario Chapter).

A. R. Troiano, University of Notre Dame (Notre Dame Chapter).

R. C. Pranik, American Screw Co. (Rhode Island Chapter).

Thomas G. Digges, National Bureau of Standards (Washington Chapter).

ALSO in accordance with revisions of the constitution adopted in October 1944, a committee for the nomination of secretary (for two years) has also been appointed, consisting of the president of the society as chairman and the six immediate living past presidents. Personnel of this committee is as follows: Charles H. Herty, Jr., chairman; Kent R. Van Horn, Herbert J. French, Oscar E. Harder, Marcus A. Grossmann, Bradley Stoughton, James P. Gill.

THESE TWO committees will meet during the third full week in the month of May. They will welcome suggestions for candidates in accordance with the ⊕ Constitution, Article IX, Section 1 (b), which provides that endorsements of a local executive committee shall be confined to members of its local chapter, but individuals of a chapter may suggest to the nominating committee any candidates they would like to have in office. Endorsements may be sent in writing to the chairman or any member of the committee.

Conclusion

This series of articles on acid steelmaking has tried to state in simple language some of the most commonly accepted melting methods, and also to describe those considered by the author to give the best steel. It has stressed primarily the importance of inclusions. Metallurgists now commonly place very little importance on non-metallic inclusions. In fact the author has heard some say that

Carbometer or Spectroscope Sample May Be Drawn Up Into Glass Tube by Suction From Rubber Bulb (*Courtesy National Tube Co.*)

the topic of inclusions and their detrimental effects has been very much over-emphasized. The author does not believe that this is true.

Let us compare the physical tests on different heats made to the same specification by five different foundries. This steel had the following analysis: 0.30 to 0.35% carbon, 0.30 to 0.50% silicon, 0.90 to 1.30% manganese, 0.05% max. phosphorus, 0.05% max. sulphur, 0.25% molybdenum and 0.50% copper. The castings were normalized at 1700°, oil quenched from 1600° and drawn back to around 1200° F. The minimum tensile specifications on the castings for this job were 105,000 psi. ultimate tensile strength, 85,000 psi. yield point, 15% elongation in 2 in., and 30% reduction of area.

One foundry cast 11 heats; only one of them passed the specifications. Another foundry had 30% of its heats rejected since the physical properties of the steel were not high enough to pass. Three other foundries which cast the job and were able to make a steel which would pass the physical requirements, made steel of noticeably different ductility. The physical properties of these three companies are listed below as averages — that is, not the best nor the worst.

Steel in all five of these foundries was made in acid electric furnaces and the analysis shot at by all five shops was the same. However, the physical properties of the steel varied considerably, as has been described. The figures are significant since the only important variable could have been the method of manufacturing the steel, and it would seem from these results that at least two of the shops have a great deal to learn about the art of steelmaking.

It is the custom in most foundries to have their technically trained men supervise the heat treating, metallographical and laboratory end of the business, whereas the actual hard sweaty work of making of the steel is in the hands of men with little or no technical training. The usual qualifications for a melter are the number of years' experience he has had in operating furnaces. It is the author's opinion that if such customs were to be altered and the making of steel put under the immediate supervision of one who has a background of technical training, any shop would profit considerably.

Since World War I there has been considerable research on steelmaking processes. An example is that done by C. H. Herty, Jr., and his associates for the Metallurgical Advisory Board at the Bureau of Mines and Carnegie Institute of Technology in Pittsburgh. This research was done in an attempt to improve basic openhearth practice and it entered quite a number of different phases of the problem. While the work was done on basic openhearth steel, it by no means follows that the results of the work are not applicable to acid electric furnaces. These pamphlets describing this work would be a great deal of help to any trained metallurgist for improving the steel he is making. There are also numerous articles on steelmaking which have been and are being currently published in the American Foundrymen's Association *Journal*. Likewise the manufacturers of the alloys used in steelmaking have many informative pamphlets to be had for the asking.

No worker in this field can afford to disregard the record of careful and extensive research done by others, and recorded in the trade literature, professional association publications, and in the technical books.

Production of Three Foundries, Working to the Same Specification

COMPANY	TENSILE STRENGTH	YIELD POINT	ELONGATION	REDUCTION OF AREA
A	114,000 psi.	92,000 psi.	16%	36%
	111,000	92,000	17	42
B	112,000	93,000	20	45
	114,000	95,000	20	48
C	117,000	95,000	22	57
	118,000	96,000	22	51

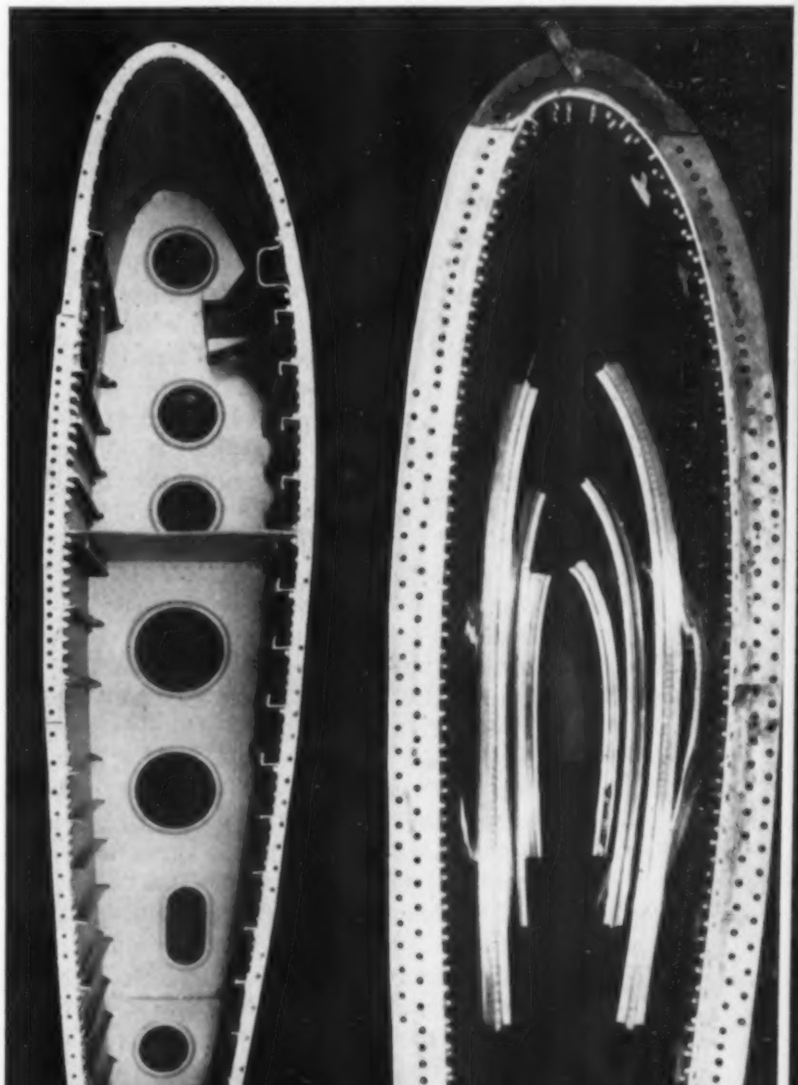
Industrial Significance of the Basic Characteristics of Magnesium*

By J. D. Hanawalt
Director, Metallurgical Dept., Dow Chemical Co., Midland, Mich.

I WOULD like to touch upon design economics, or the way in which design may determine the cost of a finished article by introducing a different production method or set of fabricating steps. The facility with which magnesium may, for example, be extruded, die cast or machined plays a large part in this, but the basic principle which often introduces the possibility of applying these operations is related to the *lightness* of the metal. This is because, if one is not aiming at a maximum weight-saving and does not have certain space limitations, he can use the extra volume of magnesium metal at his disposal without a weight penalty, and use it to *simplify* the design and the construction by increasing sections and eliminating the usual ribs and stiffeners. Such a design as illustrated in Fig. 3 has favorable service characteristics of importance, but that it has direct consequences in lowering the *cost* of an article is perhaps only beginning to be realized.

A good illustration of the principle can be seen from the figures published by Douglas Aircraft Co. regarding the magnesium floor beams used in its cargo ships. Extruded I-beams have been standard in this application for some years now. The previously used aluminum beam was fabricated from a web sheet riveted to flange angles and with stiffeners, all requiring many man-hours of labor. In magnesium, the thickness of web and flange were increased about 50% to a section. Thereupon the complete I-beam could be extruded at a good rate in an entirely feasible way, thus substituting the two machine operations of extrusion and blanking for much highly paid shop labor. The Douglas Aircraft Co. stated that the finished magnesium beam was 25% cheaper and also 35% stronger and 5% lighter than the aluminum beam it replaced.

Fig. 3 — Monocoque Construction in Magnesium (at Right) as Compared With More Conventional Construction of an Aircraft Wing. The extra volume of lighter metal can be used without any weight penalty in a thick skin, thus eliminating many ribs and stiffeners



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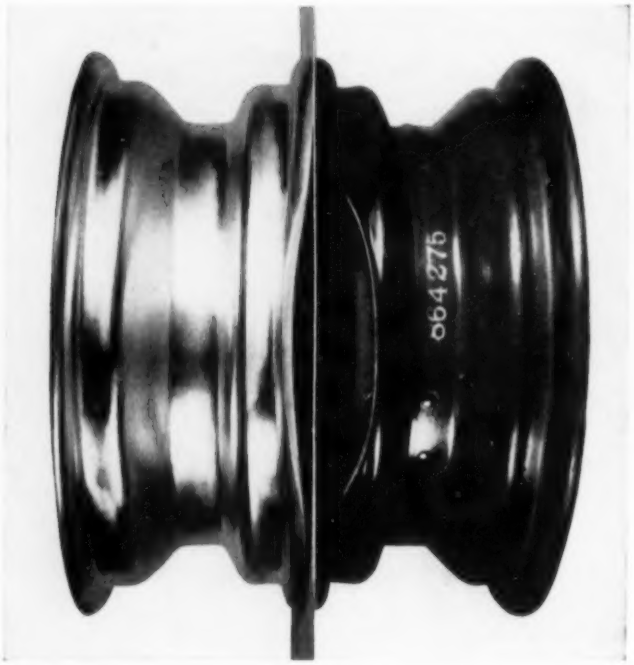
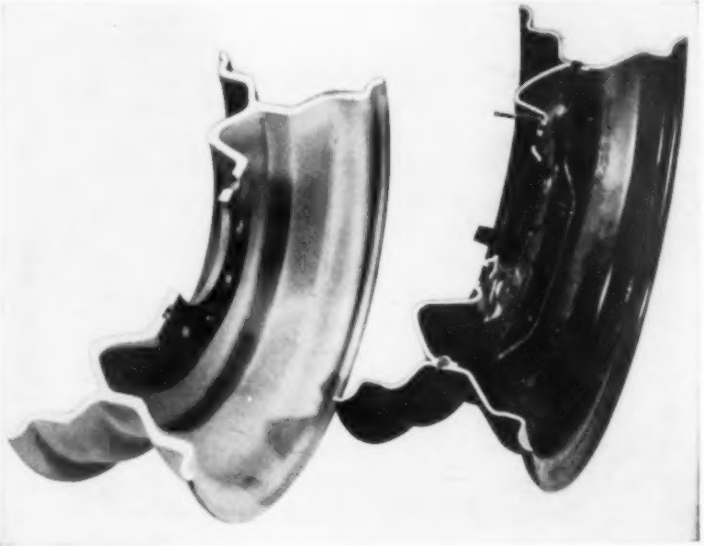
*The second portion of a paper presented before the Engineering Society of Detroit, Jan. 4, 1946.



into a wheel of twice the section thickness of the steel, yet weigh less than half as much! Because magnesium can be handled in iron equipment, we would expect a single machine-casting operation to replace the several processes requiring highly paid labor and large machinery required for the fabricated steel wheel. We would therefore expect the magnesium wheel to cost less.

The only trouble with this otherwise very fine example is that the magnesium wheel will today cost more than the steel wheel is said to cost. However, I cannot understand how the auto industry can do all those operations on steel for a relatively few cents without making magnesium wheels which would also be cheaper, if they applied the same ability to the new idea of casting the light metal. I think I can understand that, as the price of skilled labor goes up, one can afford to pay more initially for material which will reduce subsequent labor charges.

Fig. 4 (Above) — Results of Side Impact Test on 15 by 5-In. Automobile Wheels. Cast magnesium at left; weight 9 lb. Standard wrought steel at right; weight 20 lb. A 200-lb. weight had been dropped 26 in. onto the rim. Fig. 5 (Below) — Sections of the Wheels of Fig. 4, Showing Structural Design and Fabrication



Another example from the aircraft field has been supplied by the cost accounting department of another large company. The comparison is between a 60-lb. stabilizer of magnesium and a 60-lb. stabilizer of aluminum. This is an example in which the magnesium design does not aim to save weight, but rather uses greater thickness of metal in the skin structure and thus avoids the necessity of complicated and expensive stiffener construction. According to these figures, the aluminum stabilizer cost $40 for material and $191 for labor, while the magnesium stabilizer cost $62 for material but only $78 for labor.

Perhaps the most common example of design economy is that in which a different design permits a casting to be used instead of a fabricated assembly. This usually involves thicker sections. Use of magnesium simply adds the ability to do this without a weight penalty (and more generally with a large weight saving). I want to illustrate this with the automobile wheel. As designed in steel, such a common wheel as shown in Fig. 4 and 5 weighs 20 lb. The rim is rolled from sheet and closed by butt welding; the disk is a stamping; assembly is completed by riveting. Sections are too thin to be cast, and the wheel would be too heavy if the steel were thicker. Because of its *lightness*, magnesium can be cast

Unsprung Weight May Be Reduced

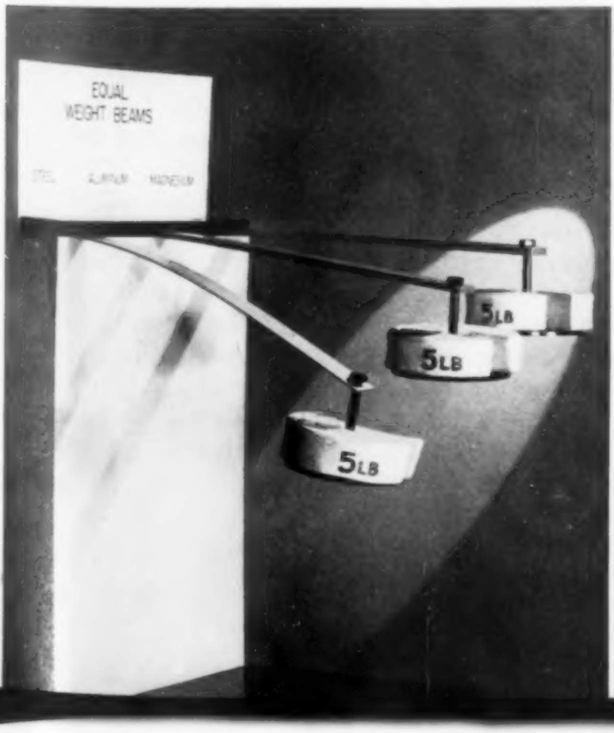
With reference to my example, I should add that if one were forced to meet a certain standard of lightness and therefore built the steel wheel to the same weight as the magnesium wheel, then I am sure that the magnesium wheel would be a cheaper part even today. This calls attention to the interesting situation that, in an era which *demands* light weight, magnesium will frequently be used in spite of other ways of attaining the requisite lightness, merely because its use will be the cheapest way of saving weight. In other words, magnesium will not be looked upon so much as being lighter as being cheaper.

One of the results of increasing the sections in a structure is so simple and important that it should be mentioned while we are talking about the effects of design.

You know that the modulus of elasticity of magnesium is 6,500,000 psi., aluminum 10,000,000, and steel about 29,000,000, so that, section for section, magnesium is deflected proportionately more under the same load. But in spite of this, structures made of magnesium can be more rigid than structures *of equal weight* in other metals, if an increase in sectional dimensions can be permitted, since the bending strength of a beam increases with the square, and stiffness increases with the cube of the thickness, while weight only increases directly with the thickness. (See Fig. 6.) In this sense, and in spite of the low modulus, magnesium structures can therefore be more rigid. This property is valuable, for example, for high speed planes where aerodynamic efficiency demands exact shape of surface, with no flutter. It is important anywhere else where the phenomenon known as "oil canning" is undesirable. Furthermore, since with thicker sections the shell is stronger, it does not need the internal ribs and supports and permits a cleaner, freer interior.

There are two interesting examples of this which are easy to remember because they are so different. Let me introduce these two examples this way: Magnesium structures have carried man higher into the air and deeper into the sea than other structures — and brought him back. In November, 1935, Capt. Stevens and Capt. Anderson made their record-breaking balloon trip of 13.7 miles into the stratosphere. Since the pressure up there is only a few pounds per square inch, they were sealed in a Dowmetal gondola. It was a smooth sphere 9 ft. in diameter, made of $\frac{3}{16}$-in. sheet, gas welded. The other record in the ocean was told to me by H. Goodwin of Birmetals, Inc., of England, which company made magnesium diving suits used in the recovery of gold from a ship sunk in World War I.

Fig. 6 — For Beams of Equal Width and Weight, Magnesium Is Stiffer Than Steel or Aluminum, Despite Its Lower Modulus of Elasticity



Each of these applications had the same requirements — light weight, yet a rigid, strong structure, simple in design, economical to fabricate, fully utilizing the interior space. While he may not be designing gondolas or diving suits, the engineer will see how to apply these principles to his own problems.

To come back to our main subject of economy, I am not going so far as to say that magnesium articles are as cheap as steel, except in the most favorable combination of circumstances, but I do believe that the additional cost for magnesium will not often be excessive for applications in which weight saving or other advantages justify some premium.

Much could be said about the value of weight saved in terms of reduced power, less wear, lower maintenance, easier handling, and so on, but that is outside of our present scope.

Serviceability

Serviceability is an important consideration from the economic as well as from other points of view. I am going to discuss a few specific phases upon which there is a great deal of misinformation with respect to magnesium. Today it is possible to clear away these misconceptions and give a truer picture of serviceability in terms of actual experience.

There is perhaps more misunderstanding about the stability of magnesium against corrosion than about any other property of the metal and its alloys. One origin of the confusion probably lies in talking about corrosion stability without adequately defining the exposure conditions.

In the average outdoor rural and industrial atmosphere magnesium is very stable. For example, bare magnesium parts will have formed a gray adherent oxide coat after 15 years, and at the most will have lost 10 to 15% of their strength — while if painted by standard synthetic paints there will be no loss in properties. Neither will continuous contact with water change these results.

An example which everyone can picture better than a test bar on an exposure rack is a canoe or a boat. More than a hundred magnesium canoes have been put into use in many parts of this country and Canada with no corrosion trouble (except for a certain stress-corrosion, which can now be avoided). After 10 years' use every summer on Higgins Lake, one of these canoes was brought into the laboratory for inspection. It has never been repainted since its original coat 10 years before and yet, except for a few places where the paint had been chipped off and worn off the bottom, the paint was still in good condition. No visible corrosion had occurred and test bars cut out of the canoe at places where the paint was chipped showed that there had been no loss in mechanical properties.

Corrosion resistance of magnesium at sea coast exposures — for example 800 ft. from the ocean on the North Carolina shore — is also satisfactory. At this exposure station are many hundreds of specimens of steels, aluminum alloys, zinc, magnesium and other metals. Within the past year or two, several hundred persons representing many American industries and technical organizations have visited this station and always have returned home surprised and impressed by the outstandingly good appearance of the magnesium alloys.

Two other factors have added to the confusion regarding the corrosion resistance of magnesium. One is the use of the salt water immersion test. The second goes back to the earlier days of inadequate foundry practice when flux inclusions in the casting turned what would otherwise have been an ordinary atmospheric exposure into a salt water attack!

Magnesium is about 0.7 volt more anodic than the other common metals. This extra voltage in contact with certain metals, either externally or when contained within the magnesium alloy as impurities, works against the magnesium when immersed in salt water or other highly conducting liquids. Consequently, commercial magnesium alloys are attacked by salt water. However, the current generated by this large voltage difference in salt water is not generated in atmosphere exposure; therefore, there is very little correlation between the results of salt water tests and the normal usages of magnesium.

With the above explanation in mind it will not be confusing to state that experiments on highest purity magnesium and magnesium alloys show that they are stable even in salt water, experiencing only surface attack after 6 years in a 3% solution of common salt.

I do not want to give the impression that we know all about the corrosion resistance of magnesium. I would rather say that we are only beginning to understand magnesium in this respect. One of the most active fields of research just now is to learn how to maintain magnesium's natural and attractive white metallic appearance. For most industrial purposes in the past, bare or painted surfaces were entirely satisfactory, but the interest in magnesium for commercial and home equipment is putting new emphasis on decorative finishes. There is much promise of the likelihood that magnesium can be finished with pleasing appearance, permanently, for many decorative applications.

Superior Resilience

A valuable service property of magnesium, not so generally appreciated, is its resilience, which is a measure of its ability to absorb impact energy elastically without permanent plastic deformation. This contributes to the practical consequence that magnesium structures, while saving a substantial amount of weight, will stand more abuse in the serviceability range with less damage by permanent deformation than either aluminum or steel.

For example, if we think of plates or tubes of magnesium, aluminum, and steel such that the magnesium is about 85% of the weight of the aluminum and 50% of the weight of the steel, and we drop a steel ball on them from increasing elevations, we get the following results: Starting at low elevations and increasing the distance of drop, the magnesium will stand a higher drop without serious deformation than either of the other two metals. This might be called the *service* range of abuse. Now, as the ball is dropped from still higher elevations, the magnesium will finally rupture while the steel will be much more deformed, although not fractured. This we might call the *accident* range of abuse, since it is of a magnitude which destroys the part.

A good illustration of this ability to stand abuse is that of gravity roller conveyers used for handling express shipments at transfer stations. In 1943, an inspection was made of ten magnesium conveyers in use at a Railway Express terminal in Manhattan. The conveyers had been in use for four years and were in excellent operating condition, though never repainted nor repaired. In 1941, because of the unavailability of magnesium, some light weight steel conveyers had been bought. The steel conveyers (weight 90 lb.) were heavier than the magnesium conveyers (weight 68 lb.), though not as heavy as the earlier steel conveyers which had been outmoded by the introduction of magnesium. In two years of service, many of the steel conveyers were in bad shape due to buckles, dents in the rollers, and rust.

The situation can be further illustrated by comparing the cast magnesium and wrought steel auto wheels under various tests, the results of which may greatly surprise you.

The first comparison was the unorthodox one of dropping both wheels from the third floor onto a cement pavement. This is not one of our regular laboratory tests, but somebody thought it up in answer to the criticism that cast magnesium was too brittle. After the drop, the magnesium wheel was undamaged; there were burnished marks where it hit, but no fractures or changes of dimensions. The rim of the steel wheel was badly bent where it struck and again dented where it hit after the first bounce.

A more scientific test was conducted by dropping a 200-lb. steel block from increasing elevations in 2-in. steps on the side of the rim, the wheel being supported in a horizontal position mounted on a vertical shaft. A regular axle mounting was used. The flexibility — that is, elastic deflection of the cast magnesium wheel — was the same as for the steel wheel but under the heavy blows of the dropping weight the steel wheel was roughly twice as much deformed at each blow as the magnesium, until at a 27-in. drop the magnesium rim developed a crack. (See Fig. 4. The steel remained unfractured, though entirely unusable.) This good performance comes from magnesium's greater resilience and the greater volume of metal in the wheel. In dynamic tests the magnesium wheels also show to advantage, having a fatigue life more than twice that of the steel wheel under the same stress loading.

In road tests these magnesium automobile wheels, as well as airborne jeep wheels of cast magnesium, have been completely satisfactory.

At one foundry in Germany last summer I met a superintendent with a vivid appreciation of the resilient properties of magnesium. At most of the places I visited, the Germans were simply waiting for someone to tell them what to do, but this man was busying himself by casting magnesium bicycle tires. He said that, with a shortage of rubber, they would soon be needed. I had to admit that — if he was determined to make metal tires — magnesium was the best metal for the purpose!

A property of magnesium alloys which should be included in any listing of important serviceability characteristics is fatigue strength. Service experience as well as a variety of laboratory tests prove that their endurance limit is relatively high. The S-N curve holds up to high values of fatigue strength with very large number of cycles of repeated stress. This is not generally appreciated as yet. In Germany some surface treatments were being used in production to increase the fatigue life of magnesium parts; present technical developments indicate still further advances in this direction.

The successful service of magnesium parts on aircraft for the past 15 years is probably well known. An aircraft engine has from 200 to 400 lb. of magnesium castings and forgings. Almost all landing wheels, whether large or small, are in magnesium. Other parts with long service records are propeller blades and fans; castings, extrusions, sheet, and forgings in the airframe, as well as seats and many fixtures.

In the industrial and commercial fields rotating and reciprocating parts of textile machines, business machines and portable tools have had a long service history. Foundry flasks made of magnesium, as well as core boxes and pattern plates, are good examples of articles combining lightness, rigidity and proven serviceability.

The service experience with magnesium in the car and truck industry in the United States is primarily limited to about 100 bodies for trucks, busses and trailers built about 10 years ago by the Dow Chemical Co., and to about 5,000,000 die cast generator and starter end-plates on Fords.

Some of the bodies are still in use; service was generally satisfactory, although undoubtedly the experience we now have in design would enable us to make better bodies today. Recently, large truck-trailer combinations have been built in California and put into long distance service. In Germany and England, magnesium has a record of satisfactory service in truck and bus wheels, engine and transmission case parts, as well as on smaller parts of passenger cars such as oil pumps, fans and pulleys.

In all of these varied fields of service, the record has shown that magnesium is a predictable metal and responds to the same principles of general design with which the engineer is accustomed to work.

I have talked about the characteristics of magnesium, the metal, but in closing let me refer to the magnesium industry. As an industry, it has a very unique and valuable characteristic, and that is the spirit of cooperation between the companies comprising it. This means that progress is more rapid because it is shared, and that the benefits of acquired experience in fabrication and utilization are available to newcomers. The complete magnesium fabrication facilities of the Dow Chemical Co., for instance, are operated to demonstrate production on a full and efficient scale, and to encourage and assist other firms in the magnesium semi-stock and fabrication business. To this end, its technical production operations are open to such companies. ◆

Bright Hardening

Atmosphere Calibration for Heat Treating Airplane Propellers

By Carl A. Liedholm
Chief Engineering Metallurgist, Curtiss-Wright Corp., Propeller Division, Caldwell, N. J.

IN AN ARTICLE in February's *Metal Progress* the pit furnaces for heat treating steel airplane propellers were described, as well as their calibration which enabled Curtiss-Wright's Central Production Engineering Department to write instruction sheets for the main operations and their variants that would insure strict reproducibility within very narrow limits. We will now proceed to describe the atmospheric controls that enabled us to heat treat these important parts without surface decarburization — or even to replace carbon in the surface layers of steel that had been slightly decarburized by preceding routine manufacturing operations such as shank upsetting or copper filleting of the internal blade edges. These latter tests were part of a general long-range investigation undertaken to improve and increase the reliability of our heat treating operations.

Preliminary Experiments — The two objectives of the following atmosphere purging experiments were to determine *(a)* the best way of introducing the atmosphere in a retort and *(b)* the purging time required for a given rate of atmosphere flow.

Since the incoming atmosphere would be relatively cold and since the effluent pipe was flush with the cover on the underside (see Fig. 1, 3 and 4 in February's article), it was thought that the most logical point for the atmosphere intake would be the bottom of the retort. Preliminary trials with the normalizing retort, however, soon showed that even the highest air : gas ratios possible (about 3 : 1) with the original low-ratio lining caused excessive sooting of the inlet pipe when it entered the furnace through the cover and extended to the bottom of the retort. Upon the manufacturer's recommendations, the atmosphere was then introduced just below the cover and sooting was greatly alleviated.

Sampling tubes, ⅛ in. diameter, subsequently were introduced at the top and bottom of the normalizing retort and the 181-cu.ft. low temperature retort to study the required purging times. Oxygen in the effluent was determined at intervals to note the progress of purging; this was considered complete at 0% oxygen for the normalizing retort, and when the effluent equaled the intake for the low temperature retort (about 0.1% O_2). Effluent from the normalizing retort was free from oxygen after 1 hr. purging time with a flow of 700 cu.ft. per hr.,★ which is the equivalent of approximately 5½ volume changes, and this minimum flow and purging time were employed during all high temperature atmosphere studies to be described. The effluent and intake into the low temperature retort had the same oxygen content after purging 1 hr. with a flow of 700 cu.ft. per hr., equal to about 4 volume changes for this larger retort, and this minimum flow and purging time henceforth were employed in the low temperature atmosphere surveys.

High Temperature Surveys

The high and low temperature furnaces, being designed for different purposes and operating conditions, had to be surveyed according to somewhat different plans and with different objectives in mind. The former was required to heat blades to 1680° F. without scaling and decarburization, and to recarburize blades that might have become decarburized in previous heating operations; the latter was required to heat blades at various subcritical temperatures where decarburization is negligible, during the intended heating times, but where a good protective atmosphere is required to prevent scaling.

The high temperature surveys were performed

★The 1-in. feeder pipe and the pressure within the 1000-cu.ft. per hr. converter limited the flow through the normalizing retort to 730 cu.ft. per hr. maximum.

in four progressive steps, in accordance with the following plan:

1. Determine the atmosphere composition required in the normalizing retort to carburize 0.005-in. S.A.E. 1010 shim stock to 0.30% carbon.

2. Determine if the introduction of a full load of propeller blades into the retort, together with the shim stock, alters the effect of the same atmosphere on the shim stock.

3. Establish a method to carburize shim stock placed *inside* one of the propeller blades forming part of a full normalizing load.

4. Determine if the atmosphere which works satisfactorily in the normalizing retort also can carburize shim stock in the hardening retort to 0.30% carbon, although the latter retort is opened and the atmosphere disturbed each time a blade is removed for hardening.

Steps No. 2, 3 and 4 included the treatment of full loads of propeller blades; steps No. 3 and 4, in addition, employed shim stock of full blade lengths inside a blade and a plate panel inside its shank. Note that the first three steps pertained to the normalizing cycle and the fourth step to the hardening operation.

Instrumentation — All high temperature surveys included the following instrumentation:

(a) S.A.E. 1010 shim stock specimens, of 0.005x4x6-in. size, hung inside the retort at levels corresponding to the blade shank and blade tip.

(b) S.A.E. 4330 blade steel specimens, $\frac{1}{4}$x4x6 in., hung in the retort alongside the shim stock to study the effect of the atmosphere on S.A.E. 4330 blade material.

(c) Glass gas sampling bottles.

(d) A General Electric dew point potentiometer with bone-dry CO_2 for refrigeration.

(e) A gas analysis apparatus for CO_2, CO, O_2, H_2 and illuminants (computed as CH_4).

Procedures — In steps No. 1, 2 and 3 of the high temperature atmosphere surveys, the cold normalizing retort was loaded, purged for 1 hr., and charged into the hot furnace (maintained at 1680° F.). Four hours later, the retort was transferred to the cooling pit, and finally unloaded at 350° F. It will be recalled that the purging time had been obtained in a preliminary experiment and the cooling time through the temperature surveys described in February's *Metal Progress*.

During each experiment the analysis and dew point of the incoming atmosphere were determined at intervals. Upon completion, the shim stock and chips from one or more 0.0025-in. surface cuts of the plate stock were analyzed for carbon. Full purging flow, equivalent to $5\frac{1}{2}$ changes of atmosphere per hr., was maintained during the heating period, but during the cooling cycle the flow was reduced just so as to exclude air.

Steel Propeller Blades Are Man-Sized Affairs Yet Work so Close to Their Limit of Strength They Require the Utmost Precision of Manufacture

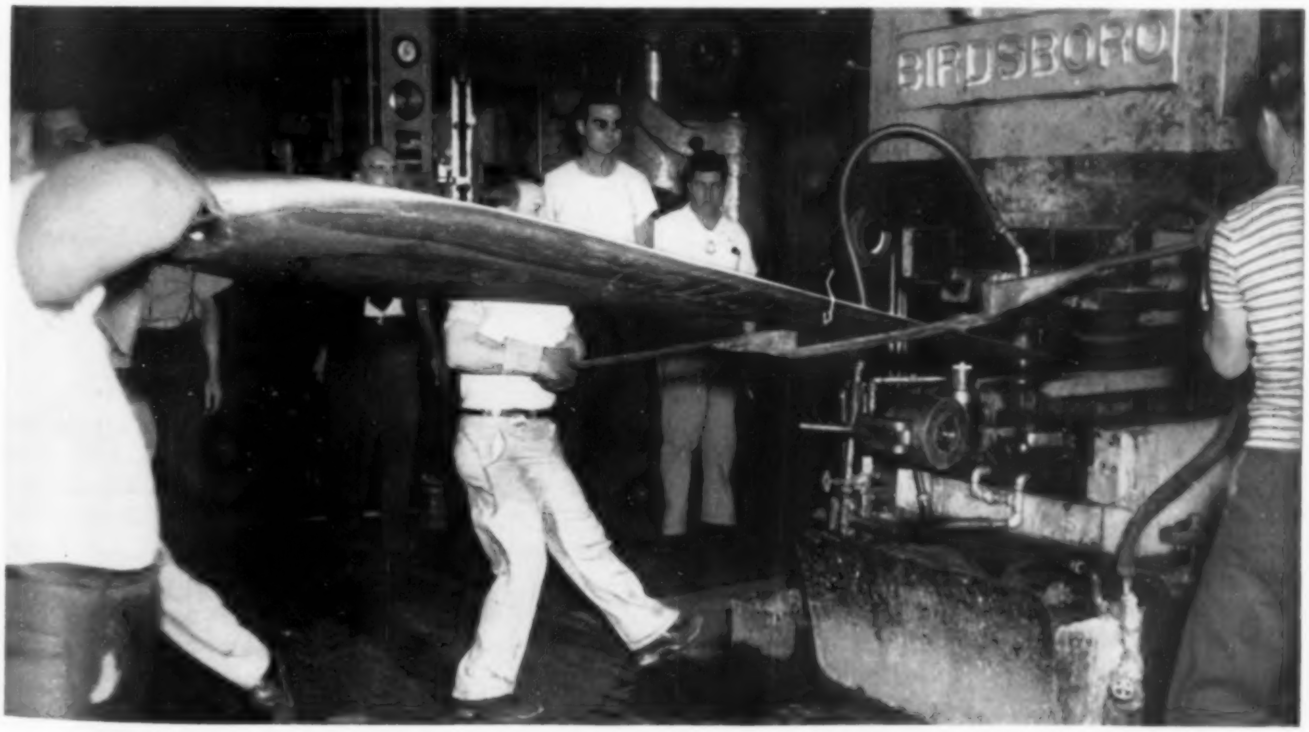


Table I — Effect of Prepared Atmosphere on Surface Carbon of Steel Normalized 4 Hr. at 1680° F.
Dew Point of Atmosphere −11 ± 3° C.

PROTECTIVE GAS ANALYSIS						CARBON IN SHIM STOCK		SURFACE CARBON IN 4330 PLATE STOCK			
								TOP		BOTTOM	
CO_2	O_2	CO	CH_4	H_2	DEW POINT	TOP	BOTTOM	0.0 TO 0.0025 IN.	0.0025 TO 0.005 IN.	0.0 TO 0.0025 IN.	0.0025 TO 0.005 IN.
						Low Ratio Converter; Air : Gas Ratio 3 : 1					
0	0	11.5	0.2	8.3	−13° C.	0.29	0.30	0.31	0.27	0.30	0.27
0	0	11.3	0.2	8.6	−11	—	0.35	0.34	0.30	0.34	0.30
0	0	11.2	0.1	8.7	−11	0.27	0.28	0.31	0.26	0.29	0.27
0	0	11.6	0.1	8.6	−12	0.30	0.32	0.32	0.28	0.32	0.29
0	0	11.4	0.1	8.6	−11	0.33	—	0.36	0.30	0.32	0.32
0	0	10.8	0.2	8.0	−9	0.30	0.29	0.28	0.27	0.31	0.26
						High Ratio Converter; Air : Gas Ratio 2.9 : 1					
0	0	10.9	0.2	8.3	−10	0.28	0.26	0.26	—	0.28	—
0	0	11.4	0.2	8.8	−14	—	0.30	—	—	0.30	—
0	0	11.4	0.1	8.5	−11	—	—	—	—	0.26	—
0	0	11.3	0.2	7.7	−14	—	0.27	—	—	0.27	—

In the two final normalizing surveys of step No. 3, one blade containing internal test panels was not purged, another was purged 5 min. with 200 cu.ft. per hr. of atmosphere, and a third such blade was purged throughout the entire heat with a flow of 100 cu.ft. per hr.

The procedure for step No. 4 was to heat the hardening retort to a uniform temperature of 1680° F. and purge it with 5½ volumes of prepared atmosphere. A scrap blade was then charged, held at the hardening temperature for 3 hr. and removed; 18 min. later, a purged blade* was charged, withdrawn after 90 min. and air cooled. This test blade was accompanied by the external and internal shim and plate panels described above. The plate panels finally were analyzed for carbon content as in the normalizing experiments.

Results: In step No. 1, the first experiment was conducted with a dew point of −43° C. and an air : gas ratio of about 2.8 : 1, the recommended "neutral" atmosphere. The shim stock and plate panels were carburized to about 0.40% carbon. In the second run, however, the atmosphere refrigerator broke down, the dew point rose gradually to a final value of about +1° C., and this time the shim stock was carburized to about 0.22% carbon. Since no other condition had changed, it was obvious that the increased dew point (that is, moisture content) had caused the change (decrease) in the carbon content of the shim stock.

The generating equipment, therefore, was altered to allow any desired portion of the atmosphere to bypass the alumina drier and, by mixing it with fully dried atmosphere, to obtain any desired dew point. Subsequent experiments confirmed that this method allowed the carbon content of the shim stock (and surface cuts of the plate stock) to be controlled at will.

During efforts to eliminate sooting of blades and retorts, the air : gas ratio had been increased to 3.2 : 1, but the composition of the atmosphere varied excessively with this mixture, even though the flowscope readings were maintained constant, and attempts to correlate dew

Table II — Experiments to Recarburize Inside of Blade in Normalizing Retort

AIR : GAS RATIO	GAS ANALYSIS						CARBON OF SAMPLE PLACED INSIDE BLADE			REMARKS
	CO_2	O_2	CO	CH_4	H_2	DEW POINT	LEVEL OF SAMPLE	SHIM	PLATE	
2.9 : 1	0	0	11.3	0.2	7.7	−14° C.	Shank Tip	0.03 0.04	— —	Blade not purged
2.9 : 1	0	0	11.8	0.1	9.8	−8	Shank Tip	0.24 0.14	0.24 —	Blade purged 5 min.
							Shank Tip	0.22 0.22	0.26 —	Blade purged during entire operation

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*Supplementary experiments had indicated that the purging of a blade took 10 min. at a flow of 100 cu.ft. per hr. The blade purging line was separate from the one feeding the furnace retort and hence the two flow rates could be independent of each other.

Table III — Controlled Carburization in Hardening Retort

Specimens Attached to Outside of Propeller Blade

POSITION IN FURNACE	TYPE OF SPECIMEN	PER CENT CARBON IN	
		FIRST CUT 0.0 TO 0.00025 IN.	SECOND CUT 0.00025 TO 0.0005 IN.
Shank	Plate, S.A.E. 4330	0.26	0.26
Shank	Shim, S.A.E. 1010	0.25	
Tip	Plate, S.A.E. 4330	0.21	0.26
Tip	Shim, S.A.E. 1010	0.22	

points and carbon contents, although revealing a definite trend, gave many erratic experimental points. Lower ratios were found to give a more reliable correlation and we finally attempted to standardize the ratio of 3:1. Under these conditions, only minor adjustments, too small to affect the ratio readings, were required to maintain the following gas analysis* which gave approximately the desired carbon content of the shim stock when the dew point was held at −11 ± 3° C. (The analysis of the city gas from which the atmosphere was generated is given under the sub-head "Low Temperature Atmosphere Surveys".)

Carbon dioxide	None
Oxygen	None
Carbon monoxide	11.3%
Methane	0.2%
Hydrogen	8.5%
Nitrogen	Remainder
Dew point	−11 ± 3° C.

Six heats run with this atmosphere gave the results assembled in the top part of Table I. The shim stock varied between 0.27 and 0.35% carbon, and the first cut of the plate stock between 0.26 and 0.36% carbon.

Following these heats, the atmosphere converter was changed to operate on "high ratio". The increased output of the high ratio machine probably resulted in a higher combustion chamber temperature, but in any event, the change necessitated the lowering of the air:gas ratio from 3:1 to 2.9:1 in order to obtain the same composition as before. Four heats run under these conditions gave the results shown in the lower part of the table. Some of these runs included full loads of propeller blades, and it can be seen that the increased load did not alter the carbon pressure of the atmosphere to any appreciable extent. The carbon content of the shim stock and of the plate surface both lay between 0.26 and 0.30%.

Two subsequent runs were made to determine a method of recarburizing the interior of the hollow blades. The first run in the normalizing retort was not purged; in the second, one blade was merely purged before charging, another was purged throughout the heat. The results are assembled in Table II. As shown, the carbon content of the shim taken from the unpurged blade varied between 0.03 and 0.04%, that in the prepurged blade, between 0.24% in the shank and 0.14% in the tip, but that of the continuously purged blade was 0.22% in both locations.

The results obtained with continuous purging were held satisfactory for S.A.E. 4330 and the final step, No. 4, of the survey was then taken. The results after hardening were as follows:*

Shank level, inside	shim stock	0.31% carbon
	plate stock	0.28
Shank level, outside	shim stock	0.25
	plate stock	0.26
Tip level, inside; shim stock (no plate stock)		0.24
Tip level, outside	shim stock	0.22
	plate stock	0.21

From these results, it is apparent that the carbon pressure outside the blade at tip level was lower than it was at shank level. This difference must have been greatest shortly after the blade was charged, since the atmosphere enters the top of the furnace and purging progresses downward from this region. Had the heat been run longer than 90 min., this difference should have been eliminated. However, slight eventual decarburization of the outside blade surface is insignificant since it is removed during the grinding operations required to balance and finish a blade. The inside, which cannot be ground, had a minimum carbon content in the tip location of 0.24%, which is

Table IV — Controlled Carburization in Hardening Retort

Specimens Suspended Inside Propeller Blade

POSITION IN FURNACE	TYPE OF SPECIMEN	PER CENT CARBON IN	
		FIRST CUT 0.0 TO 0.00025 IN.	SECOND CUT 0.00025 TO 0.0005 IN.
Shank	Plate, S.A.E. 4330	0.28	0.30
Shank	Shim, S.A.E. 1010	0.31	
Tip	Shim, S.A.E. 1010	0.24	

---

*Samples were collected over water but the volumetric percentages were not corrected to "dry samples".

*As shown in Fig. 3, in February, the hardening fixture permits the inside of blades to be purged with 100 cu.ft. per hr. of atmosphere throughout the heat. Pre-purging maintains control from the very beginning of the heating period.

entirely satisfactory. In the shank location, the carbon content of shim and plate stock was 0.30 and 0.28%, respectively. This was quite satisfactory as the plate stock specification calls for 0.27 to 0.33% carbon.

The aforementioned results can be found assembled in Tables III, IV and V. The plate stock used in the experiment originally was decarburized, hence the carbon contents of successive cuts before and after the 90-min. hardening cycle would indicate the depth of the controlled recarburization at shank level. Table V shows this depth to be about 0.015 in.

Table V — Carbon Gradient of 4330 Plate Stock Inside Blades Before and After Hardening

Depth Below Surface	Per Cent Carbon	
	Before Hardening	After Hardening
0.0 to 0.0025 in.	0.23	0.28
0.0025 to 0.005	0.21	0.30
0.005 to 0.0075	0.22	—
0.0075 to 0.010	—	—
0.010 to 0.015	—	0.26
0.015 to 0.020	0.23	0.23
0.020 to 0.025	0.24	0.25

Low Temperature Atmosphere Surveys

Low temperature surveys were performed in two progressive steps. The first stage utilized retorts charged only with shim and plate panels, and the second included panels as well as full loads of blades.

Instrumentation and procedure were similar to those employed in the normalizing surveys, except that dew point determinations were omitted and microstructural observations replaced chemical analyses.

The major difficulties consisted of sooting at low air:gas ratios and scaling at high air:gas ratios. This ratio must remain between the limits of 3.7:1 and 3.8:1, wherein slight but harmless sooting still occurred. The same atmosphere gave satisfactory results in 4-hr. heats run at 1140° F. (the draw and stress relief temperature used for blades) and at 1250° F. (the stress relief temperature used for plates). The average atmosphere and producer gas analyses follow:

	Atmosphere	Producer Gas
Air : gas ratio	3.8 : 1	—
Carbon dioxide	12.2%	3.6%
Carbon monoxide	4.8	29.2
Oxygen	0.1	0.7
Hydrogen	2.0	31.8
Methane	0.2	12.3

Approval of Equipment

Furnaces and atmosphere converters were released for use in production on the basis of these temperature and atmosphere surveys. The detailed operating instructions accompanying the release were specific and included safety rules designed to prevent explosions and other mishaps. For instance, to avoid blow-outs of the sand seals, it was required that the normalizing and low temperature retorts be lowered into the furnace at a rate not exceeding 1 ft. per min., and to prevent air from being sucked into the retorts during unloading, the load must be lifted out at a rate not exceeding 3 ft. per min. An example of the operating instructions is given in the schedule at the bottom of the column.

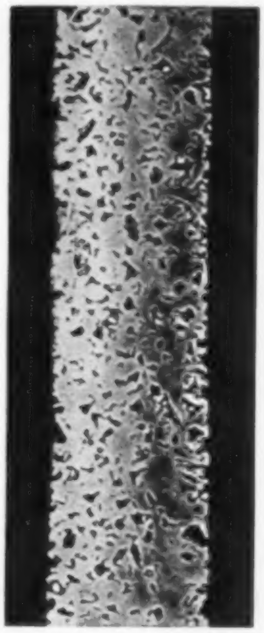
Conclusion

The city gas used in this work varied somewhat in composition, necessitating frequent analyses of the prepared atmosphere and minor

Sequence of Operations for Normalizing

1. Inspect and clean safety screen on exhaust.
2. Purge blades simultaneously with purge pipe extending to the tip. Purge with 30 volumes of atmosphere. Minimum purging flow is 200 cu.ft. per hr. per blade; corresponding time, 5 min.
3. Place hangers on blades immediately after purging to prevent influx of air.
4. Place blades in retort.
5. Inspect sand seal. Replenish with Leeds and Northrup sealing ore, 100 grit.
6. Light atmosphere below retort cover.
7. Lower cover on retort very slowly to prevent blowing of sand seal.
8. Tamp cover to form seat in sand seal.
9. Fasten cover to retort.
10. Light pilot light at exhaust.
11. Purge for 1 hr. with a flow of 700 cu.ft. per hr.
12. Shut off power to furnace.
13. Lower retort in furnace slowly (approximately 1 ft. per min.) to prevent the blowing of sand seal.
14. Light pilot light at exhaust.
15. Turn power on furnace and hold for heating cycle.
16. Shut off power and withdraw retort from furnace slowly (approximately 3 ft. per min.).
17. Place retort in cooling pit and light pilot light at exhaust.
18. Maintain flow of atmosphere during cooling cycle.
19. Loosen cover from retort.
20. Place pilot light in holder at edge of retort.
21. Raise cover quickly and allow atmosphere to burn out of retort.
22. Unload blades.

Microstructure of Shim Stock Representative of Samples Normalized 4 Hr. at 1680°F.; (Table I, Top Part); Magnification 150 Diameters



adjustment of the air:gas ratios. The ambient air varied in humidity and temperature, and this contributed further variables.

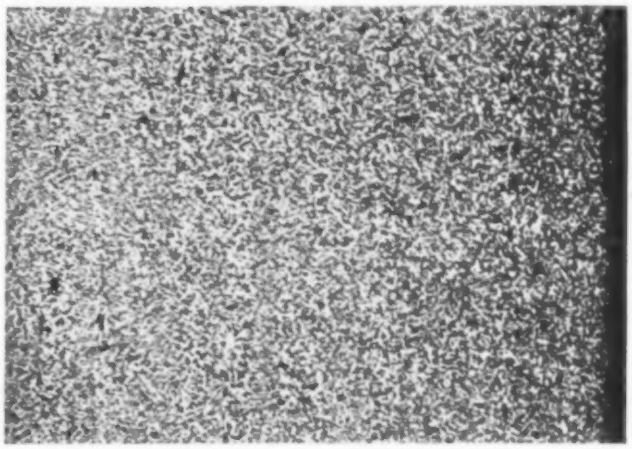
To improve the control, the equipment should operate on natural propane stored in tanks with 2 to 5 months' supply. The ambient air should also be humidified to an approximately constant dew point, and air and gas preheated to the same temperature before they are mixed. Finally, a dew point recorder should be installed to aid in keeping the moisture in the high-temperature atmosphere under continual accurate control.

All experimental work described in this and the former article, including the charging and unloading of the propeller blades, was handled by engineering personnel,* except for the operator of the converters who, however, was placed temporarily under engineering supervision and kept fully informed about the purpose and results of the experiments. The atmosphere calibration alone required the running of over 30 furnace heats. Many more hours were spent in analyzing and correcting "bugs" in the equipment. This was partly due to the unusual design of the furnaces and partly to the rigorous standards set for their performance. The temperature surveys and the atmosphere calibration each required a total of over 2000 engineering man-hours, including, of course, laboratory work such as gas analyses and carbon determinations, as well as work in the shop. However, this expense paid for itself in a comparatively short time through the elimination of spoilage and reheats, since the unit cost of the blades is high. The partial elimination of cleaning operations will result in additional savings.

Note that these experiments seemed to contradict the usual opinion that production atmospheres cannot be calibrated on loads consisting of test panels only. Perhaps it was because a high rate of atmosphere flow was maintained throughout; at any rate this work showed almost perfect correlation between results on mere test panel loads and full blade loads. The finally standardized atmosphere is believed to be in equilibrium with 0.30% carbon steel. How, otherwise, could shim stock, 0.005 in. thick, emerge with the desired carbon content after periods as long as 4 hr. at 1680° F. not only once, but time and again?† The adjoining photomicrographs tell their own story.

Carburizing control obtained in spite of the undesirable variables, as noted, depended heavily upon gas analysis and dew point tests as well as untiring attention of the operators. It is perfectly obvious, however, that unless the equipment had been carefully calibrated, and unless its future operation were kept under competent technical surveillance, "controlled atmosphere" would amount to nothing but a completely meaningless phrase. Those investing in such equipment who are not content with a dream atmosphere should first of all take cognizance of the fact that atmosphere conversion is an extremely sensitive chemical process which should never be entrusted to any but highly intelligent and trustworthy operators. They should make certain that the equipment receives the rigorous maintenance it must have to be able to perform its chemical balancing act without slipping or tiring. Otherwise, furnaces equipped with "protective" atmosphere will contain atmospheres whose effect upon the steel can be neither predicted nor controlled. ◆

Representative Microstructure at Surface of Plate Panels From Same Normalizing Runs; Magnification 100 Diameters



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*The data for this report were collected by R. M. Lacock and T. D. Nunheimer of the Engineering Materials Laboratory. Their contribution and that of other collaborating technical personnel are herewith acknowledged.

†This statement does not necessarily mean that the atmosphere constituents were in equilibrium with each other at the operating temperature.

Quench

Quenching Water Requirements of Induction Heated Parts

By B. R. Miller
Staff Engineer, Plant Engineering Dept., Caterpillar Tractor Co., Peoria, Ill.

MANY PARTS which go to make up Caterpillar products are subjected to rough usage and extreme wear. A hard, heat treated surface is therefore necessary for a part with long life.

Parts to be processed frequently require heat treatment in certain sections only; it is also desirable to use medium carbon steel. Floor space for equipment was also an important factor as the parts were to be heat treated in the machine shop. Heat radiation and smoke was also to be avoided. Medium carbon steels require a water quench soon after the heating cycle, as the allowable delay between heating and quenching is short. Induction hardening equipment was purchased, as it seemed best qualified to meet the conditions outlined above.

Very little accurate information has been available on the quantity of water required to quench a heat treated part. In the past the amount of water and pressure required was determined by experience and cut and try. As past experience was unreliable due to new and constantly changing conditions, and cut and try was very expensive, a program was inaugurated to obtain necessary data that would yield enough facts to permit the design of systems whose performance could be foretold with a reasonable degree of accuracy.

The parts quenched varied in weight from 336 to 2.6 lb. and the heating cycle from 130 to 2.2 sec. The quench water requirements varied from 807 to 22 gal. per min. These figures are very general and only outline the scope of the investigation; they mean little in themselves. For this reason several representative examples are given in the following paragraphs to show the relation of water volume, water pressure, heat supplied, duration of heating and quenching cycle, and hardness obtained, together with pictures of the parts tested.

The center main bearing of a crankshaft weighing 336 lb. was quenched (see Fig. 1). 17.4 gal. of water per quench were required at the rate of 141 gal. per min. at 23-psi. pressure for 7.4 sec. A hardness range of 74 to 86 on the Shore scleroscope was obtained. 377 kw. for 8 sec. supplied

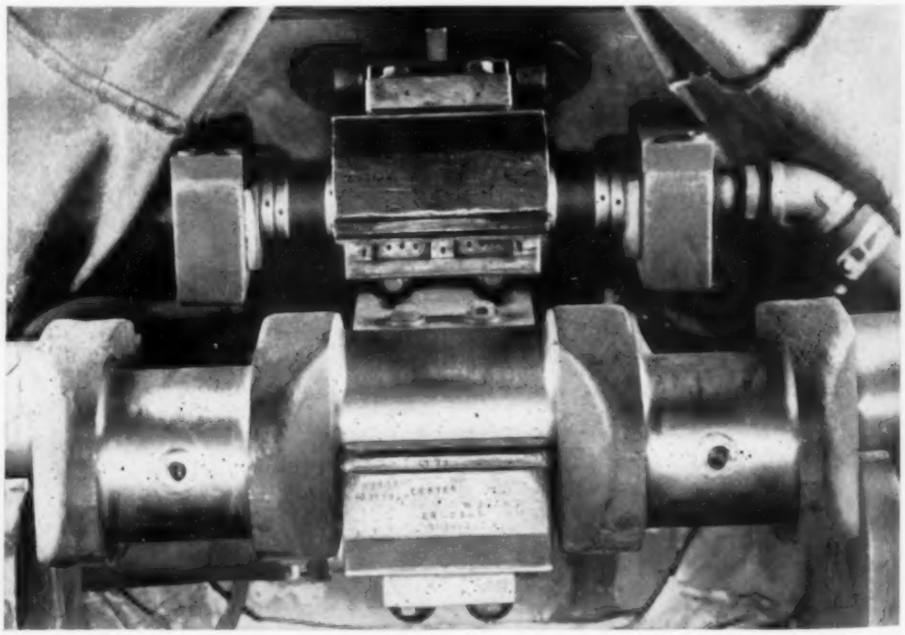


Fig. 1—Induction Heating and Water Quenching Fixture for the Main Bearing on 336-Lb. Crankshaft

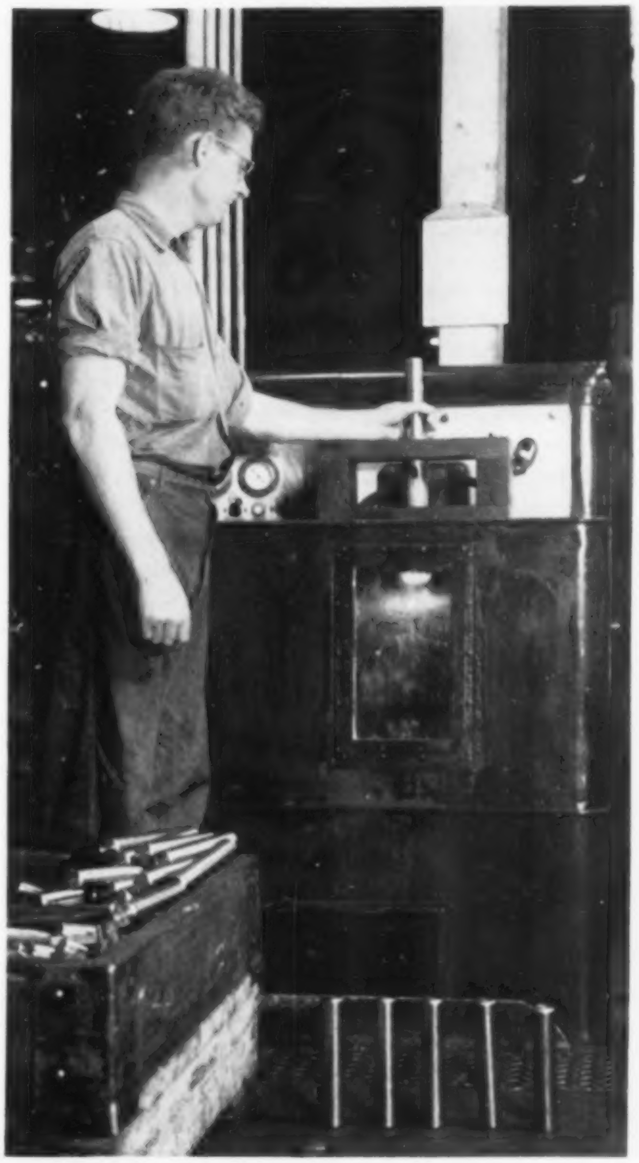


Fig. 3—Track Pins Surface Hardened During Transit Vertically Through Induction Coil and Spray Fixture

2861 B.t.u. to heat the surface. The quench water absorbed 164 B.t.u. per gal. to leave the part at a temperature of 240 to 320° F.

A final drive gear weighing 275 lb. was quenched. (This entire job was described in detail by Glen C. Riegel in *Metal Progress* for July 1943.) 211 gal. of water per quench were required at the rate of 384 gal. per min. at 10-psi. pressure for 34 sec. A hardness range of Rockwell C-59 to 64 was secured. 322 kw. for 92 sec. supplied 28,101 B.t.u. to heat the surface. The quench water absorbed 133 B.t.u. per gal. to leave the part at a temperature of 240 to 280° F.

A track roller assembly weighing 55 lb. was quenched (see Fig. 2). 101 gal. of water per quench were required at the rate of 466 gal. per min. for 13 sec. A hardness range of 495 to 627 Brinell was secured. 376 kw. for 65 sec. supplied 23,184 B.t.u. to heat the surface. The quench water absorbed 229 B.t.u. per gal. to leave the part at a temperature of 220 to 260° F.

All the foregoing classes of parts were heated and quenched intermittently. Figure 3 shows a continuous heating and quenching arrangement for track pins. The track pin weighed 2.6 lb. 52.8 gal. per min. of quench water was supplied at 6-psi. pressure. A minimum hardness of Rockwell C-60 was secured. Approximately 125 kw. was supplied to heat the surface. The pin movement was 10.6 in. per min. and the pin length was 6⅞ in. The heat supplied per pin was 4624 B.t.u. and the quench water per pin was 34.3 gal. The quench water absorbed 134 B.t.u. per gal. to leave the part at a temperature of 180 to 220° F.

The various quenching systems operating in production were utilized to obtain the information desired. Appropriate valves were closed to check the system for leaks. If no drop

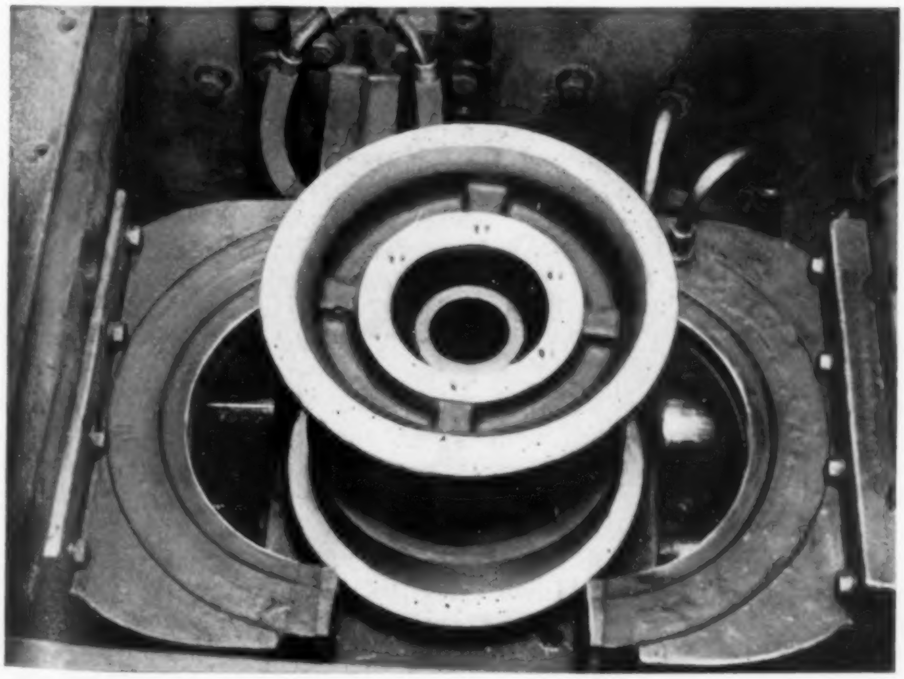


Fig. 2—Track Roller Assembly, Flange to Be Hardened. Combined heating and quenching fixture in open position

was observed in the measuring tank after one minute the system was accepted as tight. The tank level was determined. A specified number of quenches were made with only one quench fixture operating and the level determined again. The difference in the initial and the final level represented the water used in making the quench.

Two readings were obtained on each test. The pressure at which the readings were taken was approximately the same as under actual operating conditions. The float bulbs were out of commission when measuring the water so that the elevating pumps would not operate. In all tests the cold well in each system was used as the measuring tank. The valves, gages, pumps and other equipment utilized in this test were already in use on the various units.

Due to the many variables present it was necessary to test each type of part requiring heat treatment. The water requirements of various sizes of the same type of part could be estimated with reasonable accuracy after the water requirement of one was known.

This study brought to the fore the fact that quenching water pressure, volume, and the quench fixture itself are the three major items that determine the effectiveness of the quench. Additional work is contemplated to determine more accurately the relationship of the three items. Until better data are obtained, the figures already listed in the preceding paragraphs will have to suffice. The quenching fixtures used can best be evaluated by inspecting the illustrations.

This information — somewhat fragmentary, it is true — is passed on for what it may be worth to the reader with an invitation for constructive criticism or comment by those who have had experience along the same lines.

◆

Bits and Pieces

An A.S.M. book to the author of each accepted item

Handy Convertible Etch for Aluminum Alloys

IN MOST laboratories where numerous metallurgical tests are made on aluminum alloys there is a definite need for both a good macro etchant and a good micro etchant that is made up, on hand, and ready for use.

This need has been neatly provided in our laboratory by the use of what we call "Keller's Concentrate". This consists of 10 cc. HF, 15 cc. HCl, and 25 cc. HNO_3 in 50 cc. H_2O, and is kept in a wax bottle formerly containing concentrated HF. This etch, used as given, is a superb macro etch for the strong or copper-bearing aluminum alloys. By diluting 9 to 1 with distilled water, the familiar Keller's etch for microscopic work is obtained.

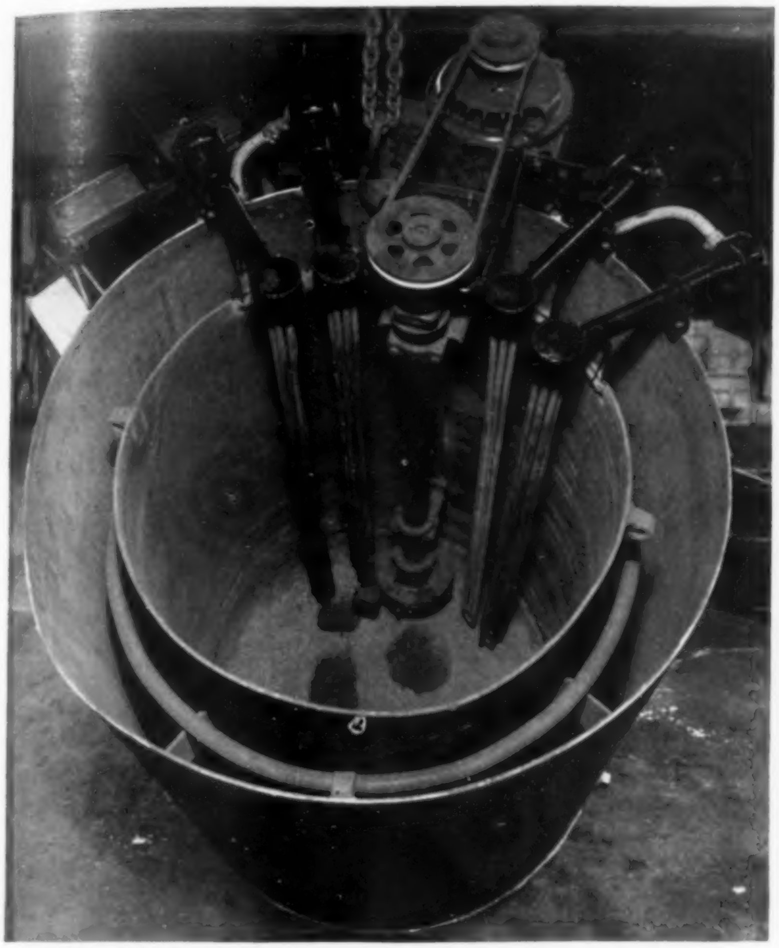
In this way both etchants are on hand in one bottle for immediate use and no deterioration of strength over long periods of time are noted, as will happen if you keep a separate bottle of Keller's standard reagent. (Lawrence J. Barker, metallurgist, North American Aviation, Inc.)

Martempering Furnace

EVEN THOUGH martempering baths are simple to build, there is a lot of discussion and interest among many people who visit our plant as to exactly how we do this. The latest tank which we have made is shown in the accompanying photograph. This shows rather clearly the details of construction of a furnace (or quenching bath, whichever you want to call it).

The inside tank containing the hot salt is 34 in. in diameter and 49 in. deep; the outside tank to catch the cooling water is 46 in. in diameter by 57 in. deep. The inner tank has a capacity, when filled to 5 in. of the top, of 2500 lb. of the low melting point salt used for martempering.

Note the exterior motor and belt drive to a vertical shaft for a circulating pump at the bottom of the salt bath. The four clusters of electrical resistors are sufficient to melt the salt from cold and bring the bath to operating temperature in 7 hr. If there has only been an overnight shutdown, the bath is back to temperature in 2 hr.



Martempering Furnace and Sump

When much steel is being quenched, the hot work brings in more heat than is radiated, whereupon water is sprayed around the outside of the inner tank from the perforated circular pipe shown near the top edge. Inasmuch as the furnace is placed in the floor we have a sump for the cooling water but most of this is vaporized immediately into steam.

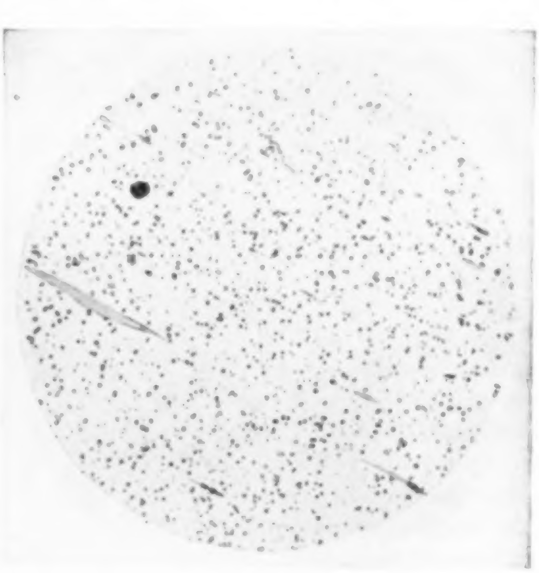
The thermocouple is concealed behind the pump shaft. It leads to controls that switch current into the resistors when temperature falls below the set point, and open a water valve for cooling water when temperature rises unduly. (ARTHUR E. FOWLER, superintendent of heat treating department, Ingersoll-Rand Co.)

Induction Hardening Ram Tapers on Steam Hammers

A SUBSTANTIAL increase in service life of steam hammer piston rods appears possible through the induction hardening of the ram taper area. A large proportion of rod failures occurs here as a fatigue fracture at or near the top junction of the rod and ram. This failure starts at the surface and gradually works inward until the section is reduced so greatly it cannot resist the service stresses.

By surface hardening the ram taper area and a section of the rod slightly above this, a compressive stress is induced in the surface metal which assists in counteracting the pulsating tension stresses of service. Increasing the hardness of this area thus improves the fatigue properties. It also provides better resistance to spalling, nicking or surface damage, and thus prevents those stress-raisers which might initiate fatigue cracks. Naturally, the rods must be completely finished and ground before hardening.

A typical rod treated as above will show a hardness of Rockwell C-50 to 55 in the case (which usually runs from $\frac{1}{8}$ to $\frac{5}{32}$ in. in depth) with a core hardness of C-30 to 35. A very large and specially designed induction heating and hardening machine is necessary for this purpose. (JAMES C. HARTLEY, director of research, Heppenstall Co.)



Unusual Microstructure (100×) in 18-8, Type 303

Unusual Microstructure in 18-8

SINCE methods of overcoming metallurgical difficulties are proper subjects of "Bits and Pieces", here is a simple determination of the nature of the unusual "structure" shown in the accompanying micro at 100 diameters of a sample of free-machining stainless steel (A.I.S.I. Type 303) etched with a solution of iodine in alcohol. The real situation was discovered as shown in the second micro [at the bottom of this page]. (W. R. RAWLINGS, metallographer, Aero Division, Minneapolis-Honeywell Regulator Co.)

Choice of Plated Coatings

IT IS a truism that metal cost is only one of many items that go into an appraisal of a plated finish on a production part. Far more important are the service requirements. Even here the appraisal may be complicated by mutually conflicting properties, and much has been published on this aspect of the plating art.

Less frequently is it emphasized that the nature of the base metal, its surface or shape, may be the controlling factor. For example, zinc die castings and aluminum alloys require special preliminary processing. Again, bright nickel and zinc plating do not work to advantage on cast iron parts. Cadmium at 95¢ per pound would frequently be cheaper than zinc at 16¢, if the shape of the part would require the superior throwing power of the cadmium baths.

Costs, in any production item, cannot be ignored. Cost of the metal plated is usually a minor item, except for the rare metals like silver, gold, platinum, palladium and rhodium. For coatings 0.001 in. thick, the following costs are representative (metal only):

Zinc	0.6¢ per sq.ft.
Copper	0.8
Chromium	1.25
Nickel	2.25
Tin	2.4
Cadmium	4.3

Labor and chemicals used in the operation usually represent more cost than the metal deposited. For example, it costs 46¢ to apply the nickel and chromium finish on an electric iron but of this amount 20¢ or 44% is for grinding and polishing, 2.7¢ or 6% is for plating materials, 6.5¢ or 14% is for plating labor, 17.1¢ or 36% is for buffing and coloring. The size, shape and special requirements of a given part are all factors in the plating cost. On a small zinc plated fuse part where the plating requirements are high and the parts intricate in shape, the metal cost of 0.6¢ per computed sq.ft. of area (costs on 135,000 parts) was less than the other chemicals used (1.3¢) and even of preparing the parts for electroplating (1.7¢). Plating labor for this part was 66¢ per sq.ft.

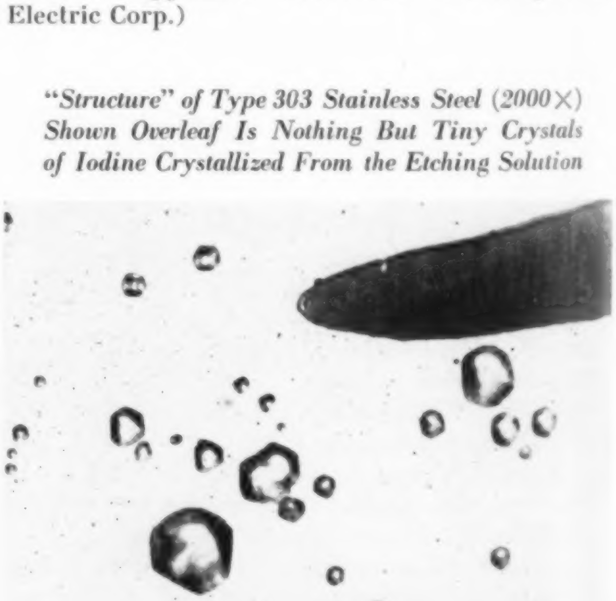
Nature of base metal affects plating costs more than ordinarily guessed. For example I quote costs, figured to a common base of 1 sq.ft. finished, of nickel plus chromium on a steel cover for an electric iron, and of the same finish on the cast iron base:

	STEEL COVER	IRON BASE
Preliminary polish	5.0¢	15.0¢
Plating materials	1.6	1.1
Plating labor	3.3	3.3
Buffing labor	10.0	7.0

Automatic electroplating equipment will pay for itself on many production jobs, but equipment of this kind requires long range planning. It is always necessary to consider all equipment necessary to produce the finish. This may involve polishing and buffing facilities, cleaning and processing tanks, ventilation equipment, generator and control equipment, plating racks, steam, water, air and floor space. The facilities will vary with the type of finish used. For example, chromium plating requires as much as ten times the current producing equipment of other plated finishes; this means additional investment in generators.

Over-all costs are *high* for electroplating. For example, on electrical appliances the approximate cost of finishing one square foot is as follows: Painting 4½¢, enameling 10¢ and plating 60¢. A high cost operation like plating must have high qualities, else the money is wasted. The responsibility for its choice and specification should rest jointly with the sales department which establishes the customer's demand, the engineer who determines the product requirements, and the manufacturing department which determines the finish it can consistently and economically produce. (H. L. FARBER, supervisor of control laboratories, Appliance Division, Westinghouse Electric Corp.)

"Structure" of Type 303 Stainless Steel (2000×) Shown Overleaf Is Nothing But Tiny Crystals of Iodine Crystallized From the Etching Solution



Improving the Formability of NE8630 Steel Sheet

By J. L. Waisman and W. T. Snyder

Respectively Metallurgists for Douglas Aircraft Co., Santa Monica, Cal., and Powdered Metal Products Corp., Chicago

MANY steel fabricators who used National Emergency sheet steels during the war encountered a considerable amount of difficulty due to their low formability — at least as compared with the equivalent S.A.E. steels. The Chicago plant of Douglas Aircraft Co. was no exception.

Prior to 1944, many aircraft parts were formed cold from normalized S.A.E.4130 on brakes and punch presses at this plant. Ratios of bend radius to sheet thickness of from 0.5 to 3 were employed.* With the substitution of normalized NE8630 steel for the same parts, a large amount of cracking was experienced. At the time this substitution was made, the rate of production was rapidly increasing and rejections due to forming cracks soon rose to intolerable heights.

This should not be considered as a condemnation of NE8630 steel sheet. It is possible that these forming difficulties were a result of production pressure. It is also possible that, with closer mill control over the cleanliness and normalizing of the sheets, these difficulties would not have been encountered. The low formability of NE8630 may therefore be due to a combination of unavoidable wartime conditions rather than an inherent defect in the steel itself. At any rate, cracking difficulties *were* playing havoc with production schedules, and an immediate method of eliminating them was needed. This article will attempt to show how the situation was corrected. It is our hope that the information presented will be of value to other steel fabricators.

An investigation of forming cracks should carefully consider the effect of production conditions, tooling, the material's surface and edge condition and its ductility. A preliminary survey of the prevalent tooling and production conditions revealed nothing that would account for the cracking. It was neither beneficial nor practicable to modify the surface condition of the material. All efforts were therefore concentrated on improving the edge condition and ductility of the metal.

All our NE8630 steel sheet was purchased to the Army-Navy Aeronautical Specification AN-S-12. Physical, chemical and bend tests on several lots revealed that, in every instance, the requirements of the specification were met. Microscopic examination of numerous lots disclosed that all sheets contained large amounts of non-metallic inclusion streamers at certain sheet levels. The presence of these inclusions in approximately the same degree in *all* lots seemed to indicate that a study of their effect on cracking would be of no quick help to us. A photomicrograph of a typical inclusion distribution is shown in Fig. 1.

We could not solve the difficulties by increasing all bend radii to some large value, since such changes would involve expensive and time-consuming re-designing and re-tooling. The elimination of cracking during forming could be and was solved by employing different thermal treatments, followed by sandblasting, prior to the forming operation. This combined treatment had the dual effect of eliminating troubles at the sheared edges and increasing the ductility of the material.

In the course of the investigation, the effect of four different thermal treatments on the formability of normalized NE8630 sheet was investigated. These four treatments, performed on the as-received† sheets, were as follows:

1. Quenched from 1550° F. into oil and tempered at 1250° F. for 2 hr.

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*The ratio of bend radius R to sheet thickness t is used here as a measure of the severity of the forming operation. The smaller this ratio, the more severe the form.

†The term "as-received" will be used hereafter to denote material in the normalized condition received from the sheet mill. The term "normalized" will denote material after normalizing at Douglas's plant.

2. Dead annealed by heating to 1550° F. and furnace cooled at 50° F. per hr. or less.

3. Normalized at 1700° F. and given a sub-critical anneal at 1250° F. for 2 hr.

4. Sub-critical annealed at 1250° F. for 2 hr. (A temperature of 1250° F. was selected since it was the highest operating temperature for available production equipment.)

All four treatments improved the formability to some extent. Treatments No. 1 and 2, however, were abandoned early in the investigation since quenching and tempering produced excessive distortion, and dead annealing required too much furnace time. Quantitative data on the effect of these two treatments on ductility are nevertheless included in the data we present below. Of the other two treatments, a normalize combined with a sub-critical anneal was found to increase formability to a marked extent, while the sub-critical anneal alone was somewhat less effective. Of these two operations, the sub-critical anneal alone (treatment No. 4) is considerably cheaper and produces less scale and distortion.

To provide supplementary data on sub-critical annealing, a series of laboratory anneals were conducted. A lot of steel was used for the tests which was B-95 to 96 in the as-received condition, B-85 to 87 after dead soft annealing, and C-22 to 24 after re-normalizing.

Samples from one sheet were reheated 2 hr. at heat and the following tabulation shows the relationship between annealing temperature and hardness. Softening of the as-received material in the lot tested begins at about 1100° F. and continues progressively until approximately 1325° F. is reached. Beyond that temperature the hardness sharply rises. A 2-hr. soak was arbitrarily selected as the standard sub-critical annealing time. In no case did the combination of temperature and time selected bring the material below Rockwell B-89.5, the minimum hardness required for normalized material at our plant.

TEMPERING TEMPERATURE	ROCKWELL HARDNESS
None	B-96.5
1100° F.	96.5
1200	94
1250	91.8
1275	92.0
1300	91.0
1325	90.0
1350	96.0
1375	98.0

In order to simplify production sequences, the policy of sub-critical annealing and sandblasting all NE8630 blanks which were to be flanged or joggled was established as a preparation for the flanging or joggling operation. Whenever possible, this treatment is performed *after* blanking or shearing so that the harmful effect of the cold-worked edge is removed. For some parts requiring extremely severe bends, the sub-critical anneal followed by sandblasting will not completely eliminate cracking. For these parts a normalizing and sub-critical anneal, followed by sandblasting, was employed.

The previous paragraphs state the general effects of the thermal pre-treatments on flanged or joggled parts. For cupped parts, early production work revealed that the as-received material had poor drawability. It was also found that material had even poorer drawability after a dead anneal. A sub-critical anneal (of the as-received material) produced a marked improvement in this property. A normalize followed by a sub-critical anneal was even better than the sub-critical anneal alone. For material which had been dead annealed, subsequent sub-critical annealing will have no beneficial effects on drawability; for such material, a normalize followed by a sub-critical anneal is necessary to produce adequate results.

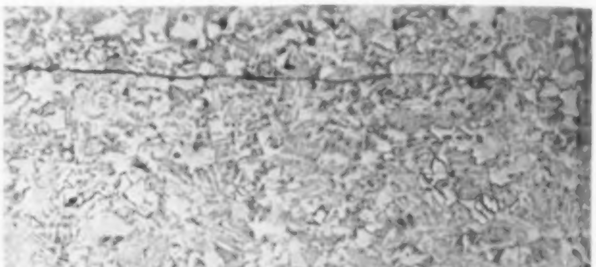

Fig. 1 — Microstructure of NE 8630 Steel Sheet, as Received, Showing Inclusion Distribution. Magnified 400 diameters. All specimens (Fig. 1 to 11) have had a light nital etch

Therefore, a standard procedure for cupped as well as flanged and joggled parts was adopted consisting of a sub-critical anneal before the forming operation. For severely formed parts and for all cupped parts which already have been dead annealed, both normalizing and sub-critical annealing is done before the forming operation.

The metallurgical reasons for the relative success of the above treatments can be seen by referring to a series of photomicrographs* of a sheet of NE8630. Figures 2 and 3 show the microstructure of a typical sheet of as-received material. Note the ferrite-pearlite banding. The pearlite is the fine, unresolved structure (at 400 diameters magnification) usually referred to as "primary troostite". The ductility (limiting elongation to fracture) of this compound structure will be determined by its least ductile constituent — fine pearlite. Thus, for flanging or joggling operations, cracking will occur when the limiting elongation for pearlite is exceeded. This would not, of course, be true for a homogeneously mixed ferrite-pearlite structure.

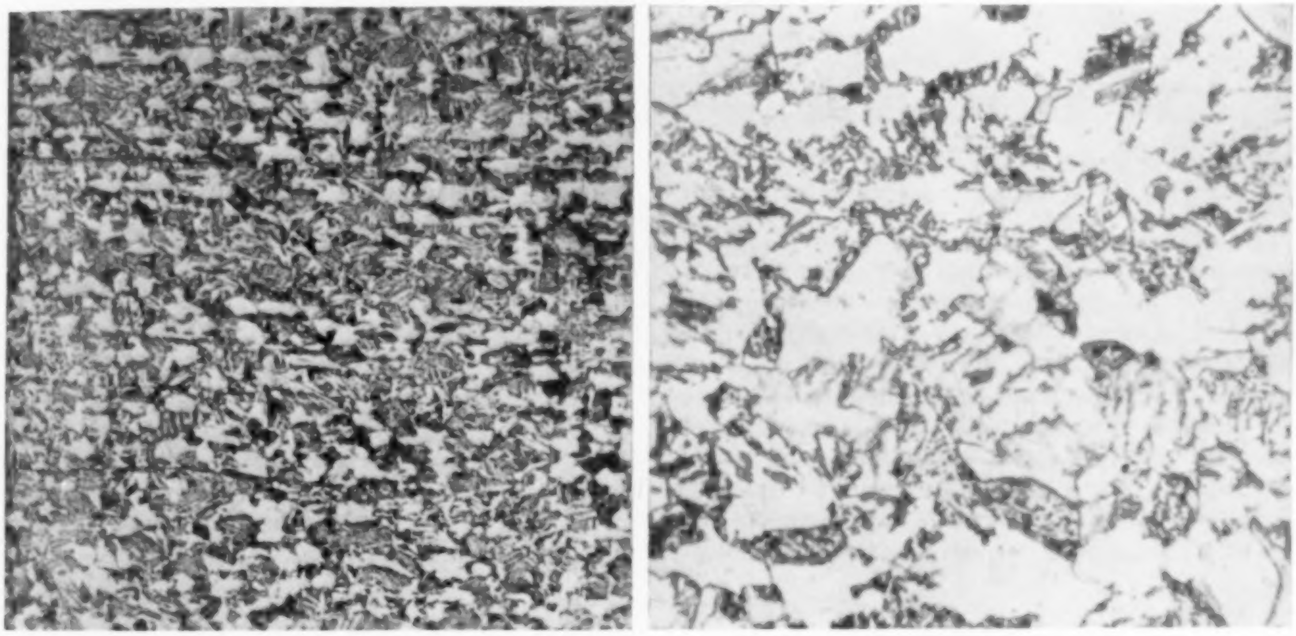

Fig. 2 and 3 — Sheet, as Received, Etched to Show Banding of Ferrite and Pearlite. Magnifications 400 and 1750 diameters, respectively

In addition to the effect of banding in decreasing ductility, another factor must be considered in the operation of drawing (cupping). During the drawing operation, the tensile stress will be greatest at the bottom of the cup, since this portion must draw the blank into the cup. The banded structure, due to micro-stress concentrations, may have insufficient strength in the transverse direction to complete a severe draw of the remainder of the ferrite-pearlite mixture. This effect may, then, cause additional premature cracking during severe draws.

The foregoing paragraphs are somewhat hypothetical and are offered merely as a possible explanation for observed phenomena.

Figures 4 and 5 show the effect of the sub-critical anneal alone on the microstructure of the as-received material. Note the partial elimination of banding and the spheroidization of the carbides. This structure could be expected to have better forming characteristics than the as-received material — a fact which experience has borne out.

Figures 6 and 7 show the structure of a specimen which has been normalized at 1700° F. in the laboratory. No banding is visible. While the pearlite itself has been dispersed in a larger number of smaller areas, it has approximately the same appearance as that in the as-received material. Normalizing alone has about the same effect on formability as a sub-critical anneal alone.

Figures 8 and 9 show the structure developed by normalizing followed by sub-critical annealing. Note that in addition to the effects described for the sub-critical anneal alone, this treatment has homogenized the structure. As would be expected from the microstructure, this combined treatment improves formability to a greater extent than does the sub-critical anneal alone.

Finally, Fig. 10 and 11 show the microstructure of a specimen that has been dead annealed. Note the highly pronounced ferrite-pearlite band-

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*All photomicrographs, bend, physical, and hardness test results shown in this article were taken from a *single* sheet of typical NE8630 material. However, production tests corroborating the conclusions have been made on numerous lots.

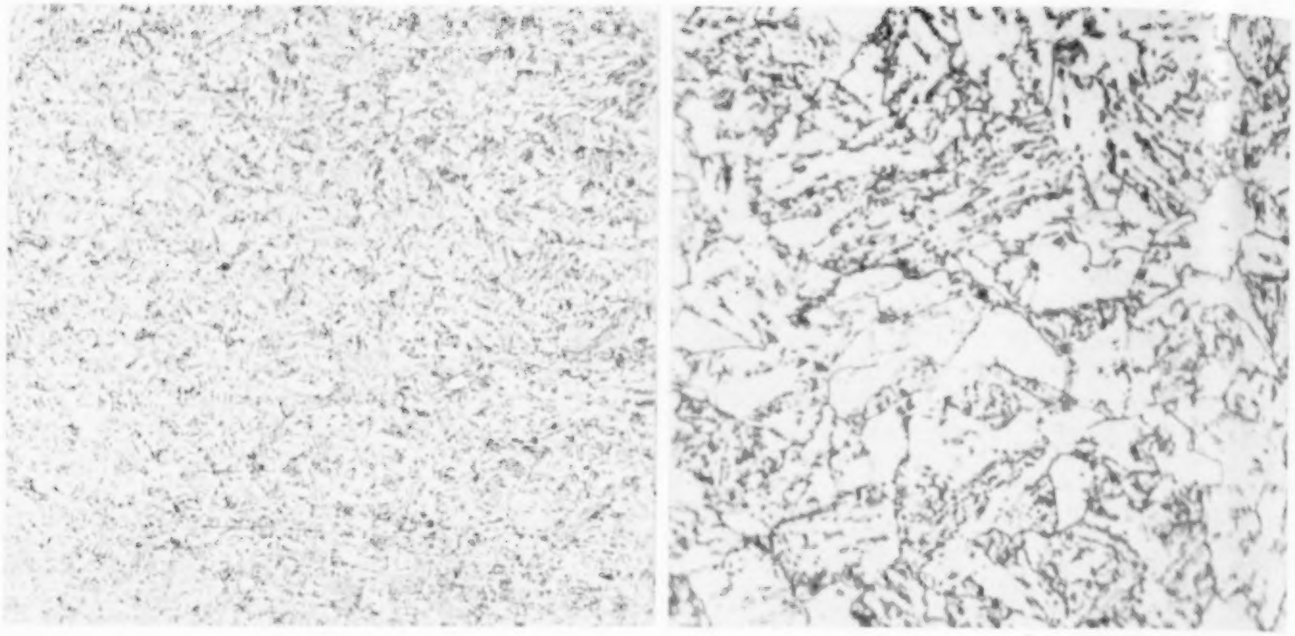
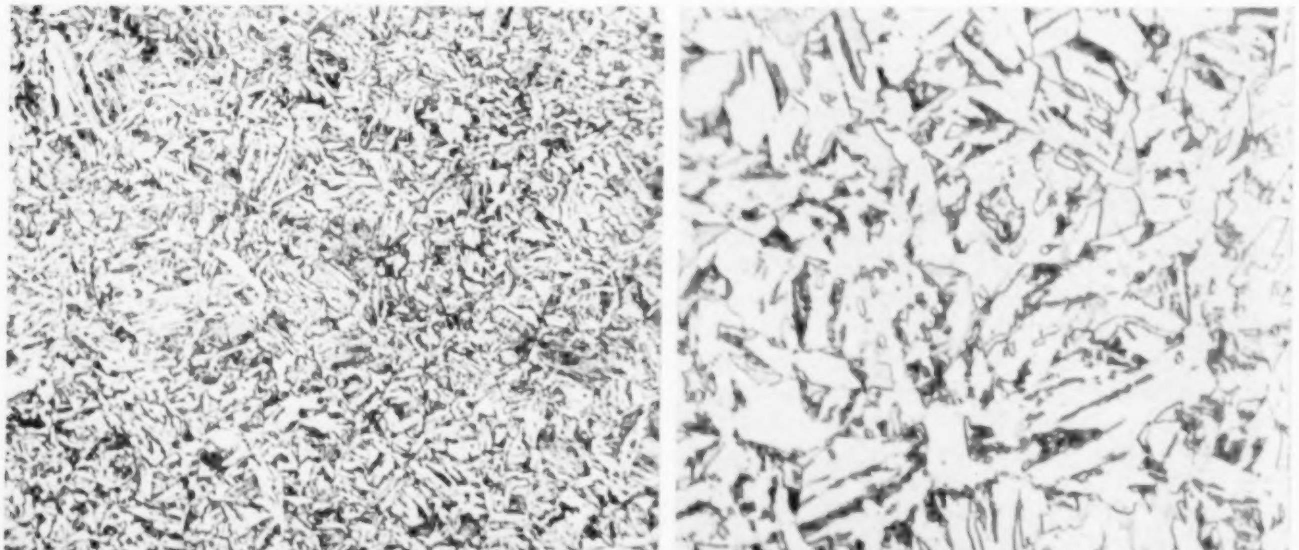


Fig. 4 and 5 — NE 8630 Steel Sheet, Mill Normalized, After a Later Sub-Critical Annealing (400 and 1750 ×)

ing. Compare this coarse pearlite with the type shown at high magnification in Fig. 7. For *flanging or joggling* operations, this treatment improves the formability of the as-received sheet to almost the same extent as a normalize and sub-critical anneal. This improvement is brought about despite the noticeable banding, for (although the banding undoubtedly detracts from the over-all ductility) the lamellar pearlite has sufficiently greater ductility than the fine pearlite in the as-received material to account for the improvement. Because of the extensive furnace time involved in this treatment, and for the purpose of simplification, annealing is not employed for flanged parts. As has been emphasized, if *drawn* (cupped) parts are made of this dead annealed steel, a large percentage of the cups will crack.

The photomicrographs reveal that the presence of banding is due to a fairly slow cooling from above the upper critical temperature. The as-received sheet must therefore have been cooled from above the critical at a rate intermediate

Fig. 6 and 7 — Microstructure of Mill Normalized Steel Sheet After Re-Normalizing (400 and 1750 ×)



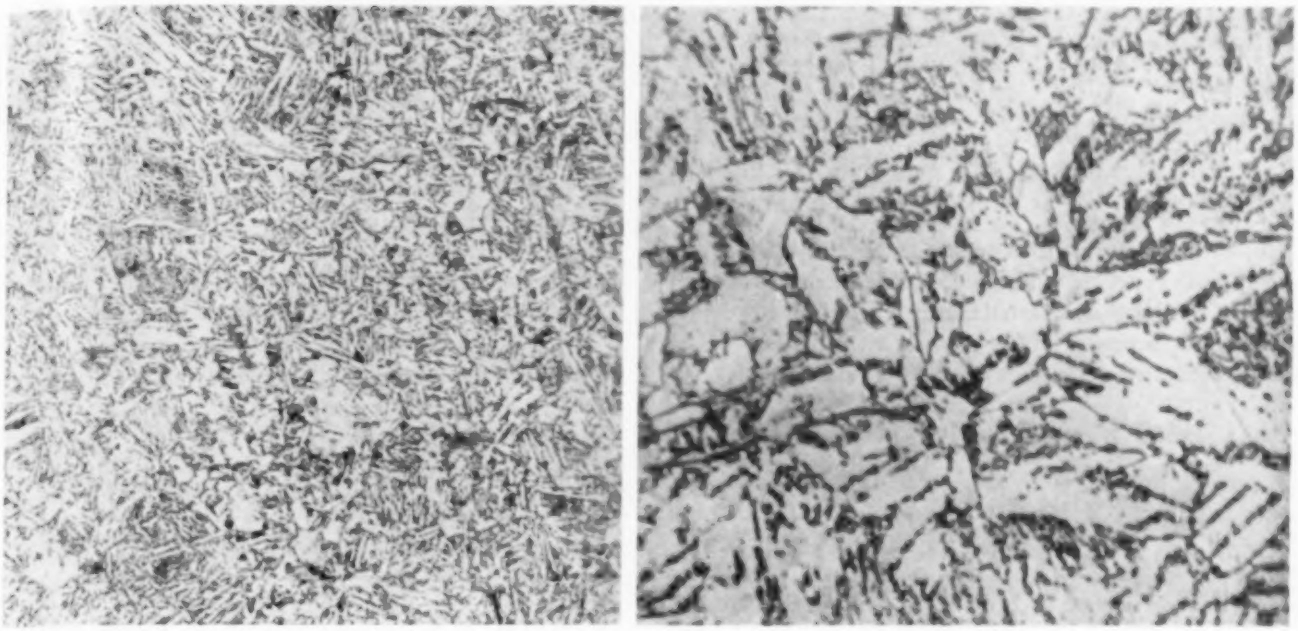


Fig. 8 and 9 — NE 8630 Steel Sheet, Mill Normalized, After Another Normalizing Heat Treatment and a Sub-Critical Anneal. Magnifications, 400 and 1750 diameters, respectively

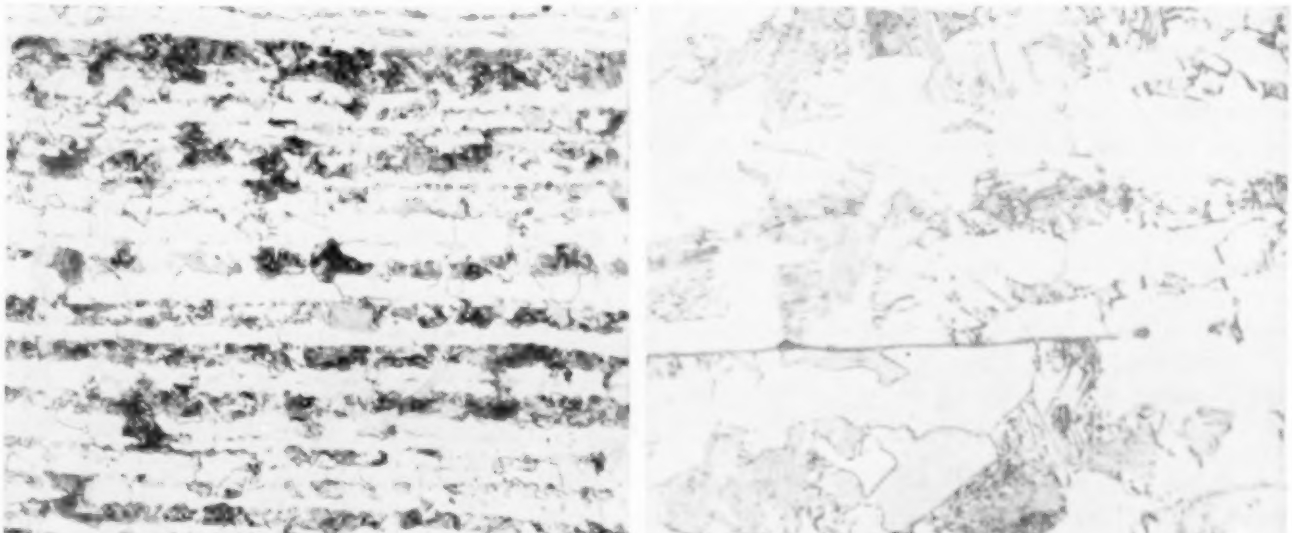
between the laboratory normalizing treatment and the dead anneal.

Most of the work upon which the standard pre-treatments are based was done on production parts. Unfortunately, in such shop tests it is very difficult to correlate results properly with physical properties, microstructure, cleanliness, and chemical composition. The following tests were therefore made in order to furnish supplementary data as a basis for quantitative comparisons.

A large number of bend tests were made using specimens taken from a typical lot of NE8630 after being subjected to variations in edge preparation and heat treatment. The lot selected complied with all requirements of specification AN-S-12 and was the same lot represented by the photomicrographs shown herewith. The chemical composition is as shown on the next page.

Table I compares the results of the various treatments on the critical bend radius and physical

Fig. 10 and 11 — Microstructure of NE 8630 Steel Sheet, After Dead Anneal (400 and 1750 ×)



Chemical Analysis of Test Lot

Carbon	0.328%	Molybdenum	0.221%
Manganese	0.74	Vanadium	trace
Silicon	0.25	Copper	0.12
Phosphorus	0.017	Titanium	0.00
Sulphur	0.028	Tin	0.011
Nickel	0.51	Aluminum	0.015
Chromium	0.62	Al_2O_3	trace

properties. Column A represents the properties of the as-received material. On freshly sheared edges without any treatment, cracking occurred at a bend radius of $4t$. It is interesting to note that sandblasting alone decreased the critical bend radius to $2t$.

Column B represents specimens of the as-received material which were normalized. Freshly sheared edges (sheared after thermal treatment) began to crack at a bend radius of $2.35t$. Sandblasting decreased this bend radius to $1.65t$, while if the treatment (normalizing) was performed *after* shearing, cracking did not occur unless bend radii were less than $1t$.

Column C represents the properties of the as-received material which has been given a sub-critical anneal at 1250° F. for 2 hr. Critical bend radii are very similar to those of the normalized material in Column B.

Column D represents the properties of the same material given a dead anneal. The effect of edge condition is seen in the figure for freshly sheared edges, which had a critical bend radius of approximately $1t$, while if the annealing and sandblasting were performed after shearing, cracking did not occur for bend radii as small as $0.65t$.

Column E represents as-received material given a normalize and sub-critical anneal. Freshly sheared edges had a critical bend radius of approximately $1.65t$, while if the treatment and sandblasting were performed after shearing, a very sharp bend radius of $0.35t$ could be used without causing the metal to crack.

Table I and the above comments are offered to show in a quantitative manner how the various pre-treatments improve the formability of NE8630 steel sheet. The values shown should not be used as definite bend radii for design purposes, but will serve adequately to give an indication of what to expect of mill annealed sheet after a definite heat treatment, and some attention to the sheared edges of the blank. As can be seen from Table I, the magnitude of formability improvements resulting from the heat treatments used is by no means reflected by the changes in tensile strength, yield, or elongation.

Table I — Effect of Heat Treatments on the Critical Bend Radius and the Physical Properties of NE8630 Sheet, Mill Normalized, 0.093 In. Thick

	A: As Received	B: Normalized	C: Sub-Critical Anneal	D: Dead Annealed	E: Normalized Plus Sub-Critical Anneal
Lowest R/t ratio for successful flanging					
Sheared after treatment	4.0	2.35	2.35	1.0	1.65
Sheared and sandblasted after treatment	2.0	1.65	2.0	0.70	1.0
Treated and sandblasted after shearing	—	1.0	1.0	0.65	0.35
Tensile strength, psi.	102,000	—	92,000	79,500	—
Yield strength, psi.	77,500	—	72,000	52,500	—
Elongation in 2 in.					
Longitudinal	19.5%	—	20.5%	—	—
Transverse	17%	—	17.5%	—	—
Hardness, Rockwell B	94 to 96	97 to 98	90 to 92	85 to 87	88 to 89

Conclusions

1. The following heat treatments produce a marked improvement on the formability in flanging or joggling NE8630 steel sheet (listed in order of decreasing effectiveness): *(a)* Normalize and sub-critical anneal; *(b)* dead anneal; *(c)* sub-critical anneal; and *(d)* normalize. For maximum improvement in formability, these treatments should be applied to the raw stock *after* blanking or shearing the parts.

2. For parts which are to be fabricated in a drawing (cupping) operation, neither the as-received (mill normalized condition), nor sheet dead annealed has satisfactory forming properties. For such parts, a re-normalize plus a sub-critical anneal has produced the best results of all the treatments attempted.

3. In terms of microstructure, best forming results are obtained from material having no banding and containing uniformly spheroidized carbides. Banding in NE8630 sheet can be eliminated by rapid air cooling of sheets from above the upper critical temperature. Carbides are well spheroidized by a sub-critical anneal. ◆

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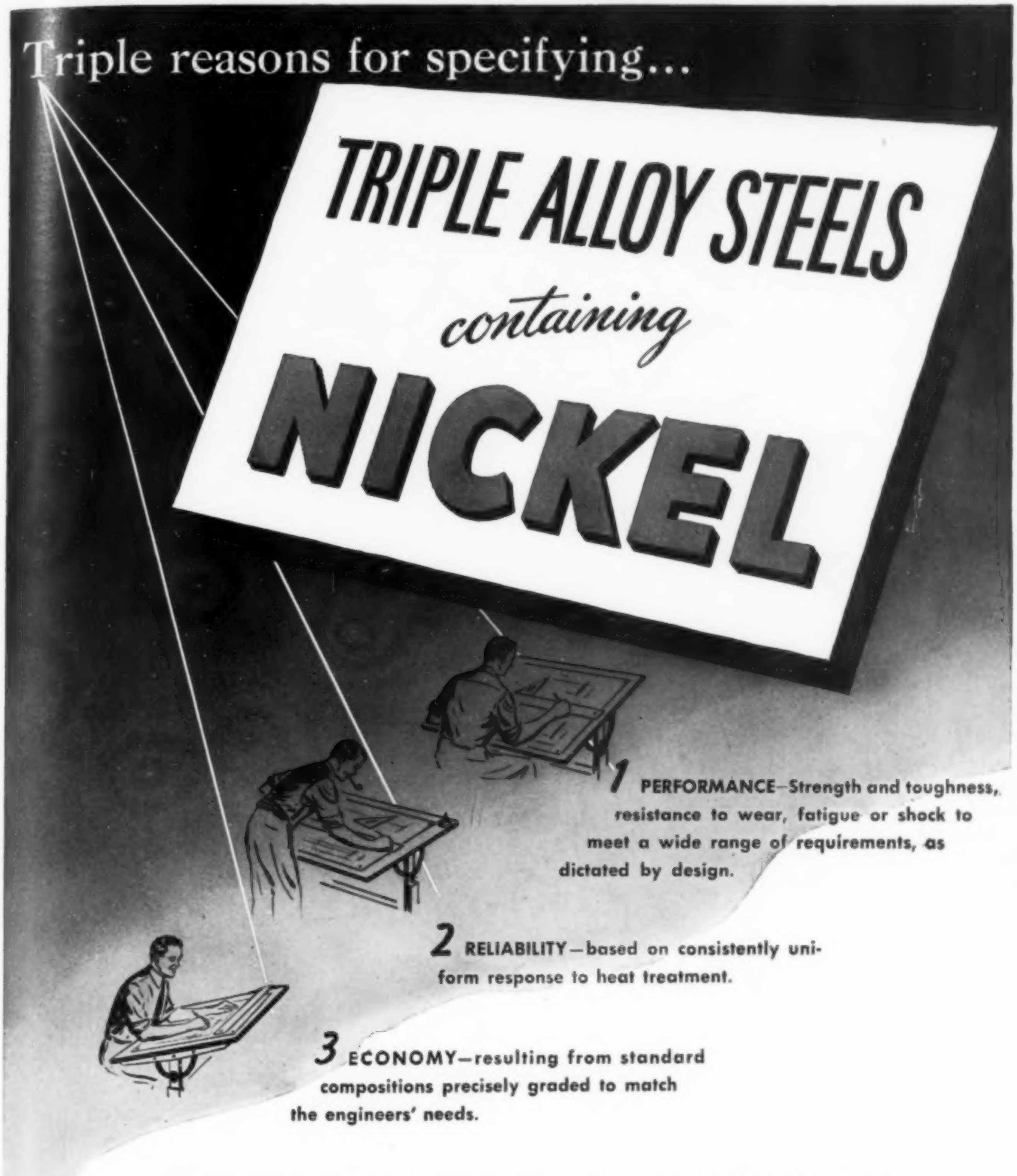

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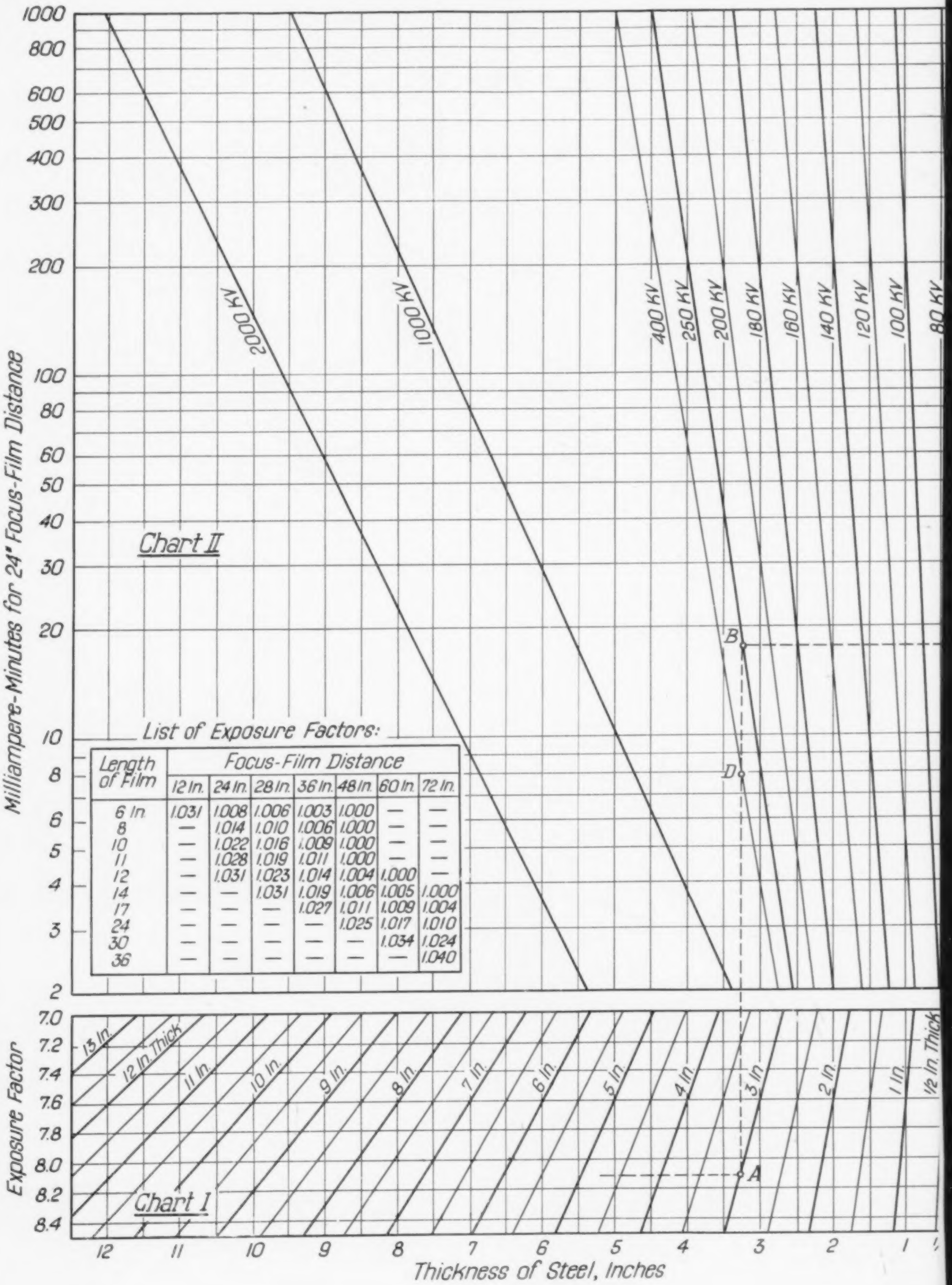


List of Exposure Factors:

Length of Film	Focus-Film Distance						
	12 In.	24 In.	28 In.	36 In.	48 In.	60 In.	72 In.
6 In.	1.031	1.008	1.006	1.003	1.000	—	—
8	—	1.014	1.010	1.006	1.000	—	—
10	—	1.022	1.016	1.009	1.000	—	—
11	—	1.028	1.019	1.011	1.000	—	—
12	—	1.031	1.023	1.014	1.004	1.000	—
14	—	—	1.031	1.019	1.006	1.005	1.000
17	—	—	—	1.027	1.011	1.009	1.004
24	—	—	—	—	1.025	1.017	1.010
30	—	—	—	—	—	1.034	1.024
36	—	—	—	—	—	—	1.040

80 KV
1/2 In. Thick

Exposure Chart for Radiography of Steel

By Herbert R. Isenburger

These charts are based on the following combination:

1. Pulsating direct current tension generating equipment.
2. Line-focus X-ray tube.
3. Industrial intensifying screens.
4. Safety X-ray film, fast screen type.
5. Five-minute development at 65° F.
6. Film density 0.7 H. & D.

The following example will explain the use of the chart:

Required: The correct exposure on 17-in. film at 36-in. focus-film distance for 3-in. plate, density 7.85, using 250 kv. and 8 ma.

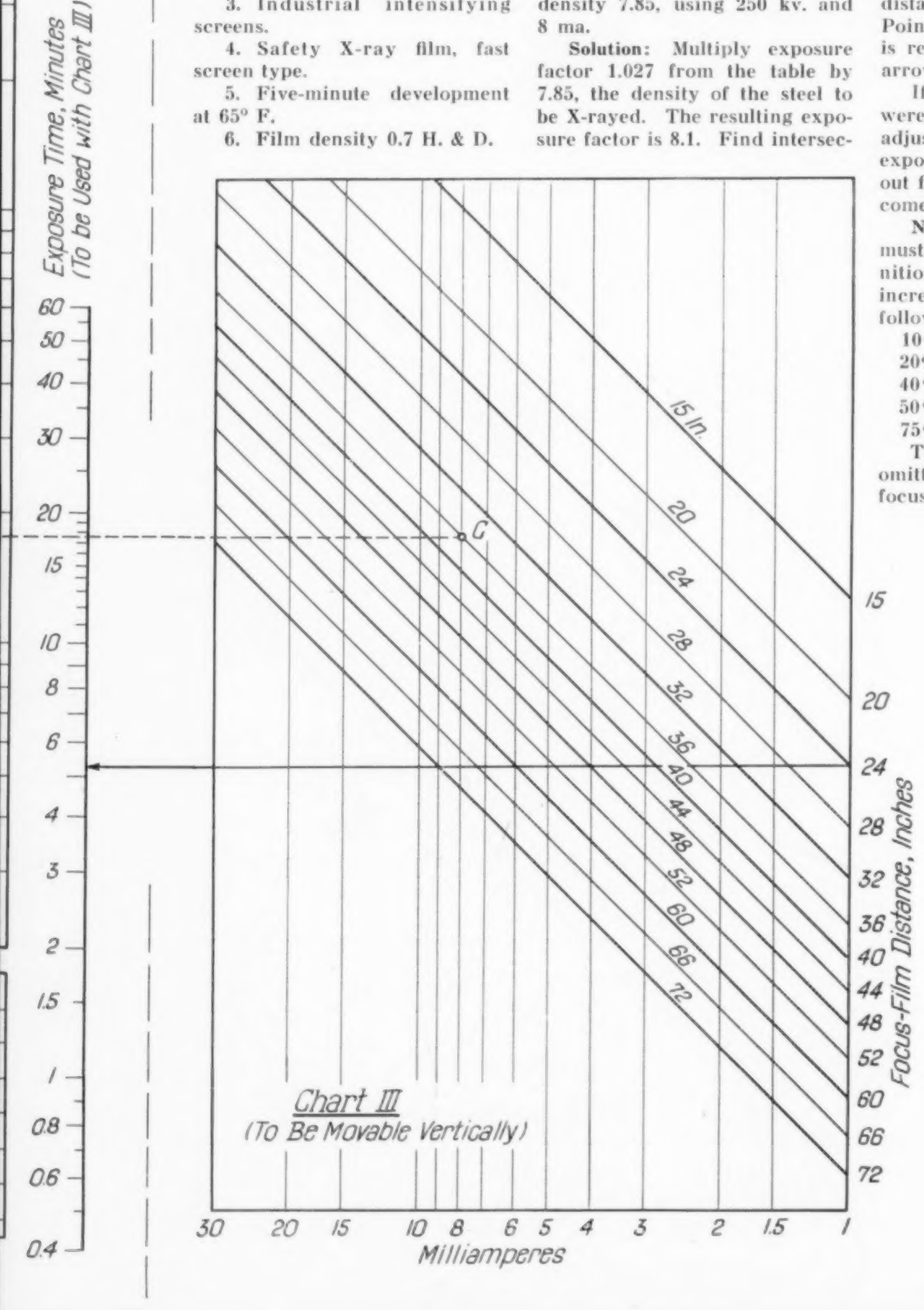
Solution: Multiply exposure factor 1.027 from the table by 7.85, the density of the steel to be X-rayed. The resulting exposure factor is 8.1. Find intersection of 8.1 with 3-in. line on Chart I (Point *A*), project upward to voltage of tube (Point *B* in Chart II). Adjust Chart III so that Point *C* (36-in. focus-film distance and 8 ma.) is opposite Point *B*, and the exposure time is read on central scale against arrow: 5¼ min.

If a 400-kv. source and filter were used in the above problem, adjust Point *C* opposite Point *D*; exposure time is 2.4 min. without filter, which, increased 20%, comes to 2 min. 54 sec.

Notes: Scattered radiation must be filtered to get sharp definition above 250 kv., and filters increase the exposure times as follows:

10% for 1½ to 2 in. of steel
20% for 2 to 3 in.
40% for 3 to 4 in.
50% for 4 to 5 in.
75% above 5 in. of steel.

The use of Chart III can be omitted when working at a 24-in. focus-film distance.



Nucleonics

Atomic Energy and Its Implications

By Cyril Stanley Smith, S. K. Allison, Zay Jeffries, A. B. Kinzel and John Chipman

VERBATIM report of a round table discussion at American Society for Metals' National Metal Congress and Exposition held in Cleveland, February 6, 1946:

CYRIL STANLEY SMITH *(Chairman)* — This evening we are dealing with the subject of nucleonics in its broad significance. On August 6, 1945, the world knew that something important had been brewing during the war years. A few people had actually seen the equally astounding test on July 16, less than a month earlier, and a small handful had seen a more important but not spectacular test on December 2, 1942, when the first nuclear chain reaction, a self-sustaining chain reaction, had been started in Chicago. The atomic bomb was the outcome of work of many researchers in universities, the outcome of the labor of many thousands of people, the outcome of enormous industrial effort. A good deal of the work that went into that has been described in rather general terms by the now famous War Department release written by Prof. H. D. Smyth and known familiarly as the "Smyth Report".*

A good many of the things that I hope we will discuss tonight will not be directly dealt with by the "Smyth Report" because they will be things which, it seems to us, will be happening in the future. A good deal of it will be guessing, a good deal of it will reflect some of the knowledge we have of the way things might go.

The group here this evening represents people who have had some connection with the project during the war. We are equally divided between people who worked in the laboratories and those concerned with the industrial aspects of the job. On my extreme right is Dr. Zay Jeffries. It is certainly presumptuous for me to attempt to introduce him to members of the ⊕ or to a Cleveland audience. As all of you must know, he is vice-president of the General Electric Co. in charge of its chemical division. During the war, he was consultant to the so-called "Metallurgical Laboratory" at Chicago University.

Immediately on my right is Prof. Samuel K. Allison, at present director of the new Institute for Nuclear Studies at the University of Chicago, and who, during the war, played a very prominent part in the work on the atomic reactions done at Chicago, for several months being director of the metallurgical project there, and later became assistant director of the Los Alamos project.

On my left is Dr. A. B. Kinzel, vice-president of the Union Carbide & Carbon Research Laboratories and the Electro Metallurgical Co. He was associated with several different aspects of the work during the war, not only on Union Carbide & Carbon Corp.'s work at Oak Ridge and elsewhere but also as consultant to the division leaders in charge of chemistry and metallurgy at the atomic bomb laboratory situated in Los Alamos, New Mexico.

John Chipman, at my extreme left, is professor of metallurgy at Massachusetts Institute of Technology. He was associate division leader at Chicago, in charge of the metallurgical work there and director of the metallurgical sub-contract at M.I.T. in Cambridge.

Having introduced everyone else, I suppose I might introduce the chairman. I am Cyril Stanley Smith, now director of the Institute for the Study of Metals at the University of Chicago, and during the war, from April 1943 on, I was in charge of the work on metallurgy at the Los Alamos atomic bomb laboratory.

I think it is best for us to start with a discussion of the physics involved in nuclear reactions in general. I will ask Mr. Allison to start this by some discussion of the principles of nuclear energy and its release.

ALLISON — Well Cyril, there seems to be, in spite of the "Smyth Report" and in spite of the

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*A 32-page condensation of the Smyth Report was printed in *Metal Progress* last December; reprints are available on request.

wide amount of publicity and discussion on this subject, some fundamental misconceptions abroad. In Chicago, a month ago or so, I talked to some chemists (I won't admit that there were any physicists there!) and one of them said, "This energy release that you fellows got out there in the desert is really impressive. You really must have found some way to speed up the oxidation of uranium awfully fast!" (This remark was made *before* my speech, I may add.) I am sure that a great majority of you know and realize that the sources of the energy which we released come from an entirely different part of the atom than is involved in any chemical reaction.

The picture of the atom which we hold today has been with us since 1911 or so. In the *interior* of the atom and in its approximate center is the source of the power that we are talking about this evening. The apparent diameter of this so-called nucleus is only about one hundredth of 1% of the diameter of the space occupied by the rest of the atom, but it does contain the mass of the atom. The external part of the atom, relatively far distant from this nucleus, contains the electrons whose interchange gives rise to chemical reactions such as the oxidation of uranium, and the burning of coal with which we are heating this hall, and the more complex chemical reactions which sustain life in our bodies. Now, all of those combinations and interchanges lie in the region of the atom which is completely different from the region from which we draw this new *atomic* energy.

(Of course, we have made a mistake in naming the thing. It should be called nuclear energy to show that it comes from the nucleus, but President Truman called it atomic energy and that settles that. He should not be blamed for this. His speech was probably written for him by someone who should have known better.)

Nuclear Energy Rather Than Atomic Energy

Anyway, there we are! We are dealing with a source of energy which is not at all related to chemical energy. It comes from an entirely different part of the atom and is released by methods which are not at all typical of chemical methods.

We physicists were disintegrating atoms in our laboratories long before the war. We were liberating energy from the nucleus on a very small scale, an infinitesimal scale, when we were splitting atoms. I would like to make this point to show what happened that enabled us to evolve large amounts of energy — really enormous amounts of energy — and what caused the great jump from our old laboratory experiments to the energy we are now able to explode. I don't think the Smyth Report quite makes this clear. (It is no wonder because poor Mr. Smyth had a hard time with that report. He wrote it and then the Army deleted parts which ought to be suppressed and the report was therefore left somewhat mangled.)

You see, the nuclei were being disintegrated with apparatus known as the cyclotron before the war, and this required high voltage circuits and the use of charged particles. That is to say, we accelerated charged electrical ions in the cyclotron, fired them into solid targets, and these charged ions would occasionally (very occasionally) hit a nucleus of an atom in the target. This impact would then cause the disintegration of that one nucleus and usually another particle would fly out of the nucleus and energy was released. Such a fundamental experiment was popularly familiar as "atom splitting".

"Well, now," you say, "as long as that happened, why didn't the whole target disappear; why didn't the nuclear disintegration spread through the whole mass of material the way it does in uranium?" The point is that *charged* particles, flying about in matter, usually can't get at a nucleus — they simply fritter away their energies by banging against the charged electrons which comprise most of the matter and which act as a barrier surrounding the nucleus, so that the vagrant particles are wasted and do not propagate or transmit this message of disintegration from one nucleus to the other. One action doesn't reproduce itself, let alone multiply and build up in the solid material.

What changed all of this picture was the discovery of a new type of nuclear disintegration, which we now call "fission" because it is typified by a heavy nucleus like uranium splitting into two almost equal portions. This new action is caused (now notice carefully) by ***neutrons***, which are uncharged — particles which carry *no* electrical charge — and the fission of uranium emits more neutrons which also are uncharged. These neutrons can wander through solid material, paying no attention to the electrically charged electrons which abound there; they have no means of interacting with the electrons because the neutrons have no electrical charge. The only thing a neutron can do in life is to attack another nucleus because it is insensitive to purely electrical forces, and only sensitive to the nuclear forces which are in some ways very different. This is the key to the situation.

Let me repeat it again because it is a little subtle and not easily grasped: The peculiar situation which we have here is *(a)* that we have a nuclear disintegration which is caused by neutrons. When a neutron, which is an uncharged particle, attacks a uranium nucleus the uranium nucleus splits into two parts, roughly equal in mass, and then *(b)* a product of this splitting is some more neutrons which can travel on through the solid material and cause further disintegration.

The emphasis in this whole discussion is on the *neutron*. Its peculiar properties allow the disintegration to propagate itself, to build up and spread through the mass of material until enormous amounts of energy are ultimately liberated. Just like an unexpected inheritance, it turns out that the amount of energy liberated in this peculiar type of reaction within the uranium nucleus is very large, even on a nuclear scale. That means that it is fantastically large on a chemical scale.

In some places I have met the misconception that this uranium business goes because the energy evolved is so large. That is not true. It goes because of the peculiar neutron-linked reactions that I talked about, and not because the liberated energy happens to be very large. It is just so much "gravy" that the energy turns out to be a thick, generous slice.

I might give you a few numbers to go by. If you make a reaction system in which uranium keeps breaking down in this way, and it is putting out 1000 kw. for 24 hr. thus generating 24,000 kw-hr. of power, that represents the transformation by fission of roughly one gram of uranium. One gram of material to run a sizable rolling mill for three shifts! In that transformation, the uranium atoms are merely split apart. The amount of mass that has disappeared into energy is enormously smaller than that one gram, yet one gram of uranium is less than the size of your little finger at the fingernail end.

Let us take another illustration. The United States burns about 660,000,000 tons of coal per year to keep us warm, happy and transported. This much energy could be supplied by the breakdown through fission of about 240 tons of uranium. Uranium is pretty dense material (and we are going to hear a little about its metallurgy later I hope); its density is about 19 times that of water. A cubical box about 7½ ft. on an edge would hold 240 tons of uranium. That rather insignificant object would supply enough fuel to run the United States for a year!

Another hackneyed illustration is that 1 kg. (roughly 2.2 lb.) of uranium which is transformed by fission liberates about the same energy that you get from the explosion of 17,000 tons of TNT.

Those figures alone show quite clearly that we are dealing with something which is quite out of the range of chemical reaction, something which is completely fantastic in terms of chemical energy, and we are opening the door to a new and quite unprecedented source of energy.

Let me talk a little bit about controllable releases of energy through the "chain reaction", as we call it. This chain reaction refers to the neutron-linked chain in uranium. It is now possible (and has been since 1942) to release energy in an orderly, controllable, and reproducible manner without hazard from this chain reaction. The control is very elegant. It works beautifully. It is not at all dangerous if you build the machines properly.

The controllable machines which have been working for some time contain uranium and graphite (as you probably have read). The graphite has to be rather special stuff, quite pure. The uranium is in metallic form — and there comes in metallurgy. The uranium is equally distributed in lumps throughout the graphite structure like a lattice.

Now, the reason for the graphite is a little abstruse, but it is really to slow the neutrons down. The neutrons as they come out of the splitting uranium nucleus are quite energetic (having the equivalent of several million volts of energy) and in order to make the thing run with normal uranium you have to slow those neutrons down rapidly and get them down until they are staggering around, so to speak, with little more than the energies of thermal agitation. They can then be effectively and rapidly absorbed by U-235. (It is the isotope of uranium having 235 mass units, and which is a constituent of uranium as found in nature, which is essential to the chain reaction as it goes on in the uranium metal derived from uranium ore.)

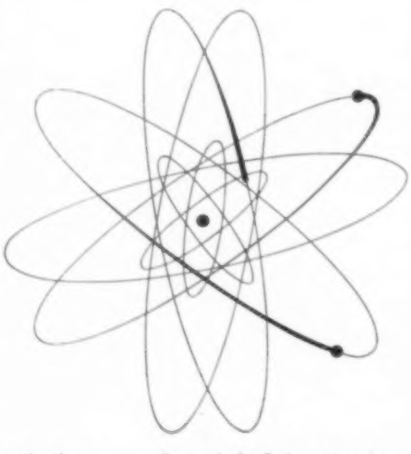
A Model Power Plant Impossible

There is one curious feature (there are several curious features, but this is a queer one) of our nuclear chain reactors and it differentiates them from any other type of mechanism which produces power, and that is that you cannot in principle and you cannot in reality make a model of a nuclear chain reactor which produces any power. This is a curious feature because if you want, in this day and age, to convince somebody that a steam engine would work and produce power you would probably rig up a little condensed milk can and put a candle under it and drop some water into it. You would let the steam blow out of the round hole that the milk originally came out of, and let the steam jet hit a pin wheel. The thing would work; the wheel would hum around; it would look as if something could be done with the system. Mark this — such a model is, in principle and in reality, impossible with atomic power machines. They are all characterized by a curious phenomena known as the critical size. You can get samples of uranium and graphite and put them here on the desk and nothing happens. The uranium is quite inert and only the most refined instruments would detect that there was any activity going on inside. You couldn't get any power out of it for love or money. You couldn't do anything with it until the size gets big enough.

That, of course, was a great hurdle that had to be jumped. Although we could sit down and spin differential equations which indicated that there might be atomic power developed, you couldn't convince the skeptical man on the street by any demonstration. We had to have at least the critical size, and in the machine using natural uranium the critical size requires many, many tons of uranium, yet when we started we only had milligrams of the stuff in metallic form!

The reason for the critical size can be stated rather simply: If you don't have a structure big enough, the neutrons, instead of wandering around and causing fission in another nucleus, wander out to the sides and escape. In a small structure there is too much surface area in proportion to the mass. A neutron cannot participate in the next disintegration if it flies out into the air. There is just nothing you can do about it if the thing escapes — you have lost it. Consequently the evolution of power cannot begin until you have a very large structure containing many tons of uranium and many tons of graphite. You have got to persuade someone to put up the capital to produce those materials on the faith of looking at a few differential equations on paper, because you cannot prove to him by any gadget that the thing is going to turn out any power. For this necessary financing you really have to thank President Roosevelt, because he was approached in 1939 and told that we had gone through these calculations as to what was likely to happen. We couldn't prove it to him; we couldn't show him a sample, but it was in the cards. It was through his action that the rather large sums were appropriated to get us over the first hump, showing that the thing would actually turn out power, and it took a lot of money to reach the first critical size.

Now, what happens when you reach the critical size? You keep building this combination of uranium metal and graphite bigger and bigger, and when you come to within 90 or 95% of the critical size you will notice, if you have detecting equipment properly installed, that a few neutrons liberated at random spots build up into a big crowd of neutrons inside. Then the crowd, as counted by proper detectors, levels off at a fairly high number in each unit of space, but the mass is still stable; the numbers do not increase beyond that high level and it still does not evolve any power. You can summarize the situation like this: As you get nearer and nearer to the critical size, the thing begins to act like a huge magnifying glass — it will multiply many times any neutron you put in but still the image doesn't go beyond all bounds. When you reach the critical size and get a little bit beyond, however, you find that the number of neutrons in unit space within the mass is growing, slowly but steadily, in this structure. The first time we approached the critical size of the "piles" as we called them, we were so scared of the stuff that we added amounts of uranium metal and graphite so gingerly that the intervals ran to hours. We sat around and watched the thing and five minutes after the final pile was assembled there would be twice as many neutrons as there were at the first observation; after 10 min., four times, after 15 min., eight times, and so on. Obviously if you don't do something, it will go beyond all bounds and the energy released will be so great as to melt down the structure. We had to control it somehow, and we controlled it by sliding into the structure something that robs it of neutrons — just like making space on the inside for the neutrons to disappear into. We put in something that, for instance, contains boron (since boron atoms absorb neutrons without important aftereffects). When you can stop this evolution of more and even more neutrons by taking some of them off, you have a control. By putting this control rod in when things get too vigorous you keep it within bounds; if you want to develop a little power, you pull it out again.



Another of the characteristic features of nuclear phenomena is their reliability and their reproducibility. Anyone who has tried to start an automobile on a cold morning has had more trouble than anyone who has tried to start a chain reaction. They always start! If they didn't start there would be something curiously wrong with the universe; it would be something like getting star dust on the cosmic spark plugs!

You have probably heard a lot about the separation and purification of uranium. That will probably be touched upon later. You have also heard about production of plutonium, a new element. The question might arise, "If you can get atomic power out of natural uranium, why go to all the agony of producing these new things and what's the point?"

Let me introduce the answer like this: In a structure made of the mixture of isotopes that is natural uranium, the most abundant type of uranium, isotope U-238, acts as a sort of poison. It robs neutrons itself and prevents them from getting back and attacking more U-235 nuclei and carrying on the reaction. This means that in the graphite-uranium system, the U-238 uranium is actually undesirable for carrying on the chain reaction because it laps up neutrons and is a sort of a control rod that you can't pull out. One way to better this situation is to remove some of the U-238 leaving more and more U-235. Another "out" is in the production of this new element plutonium from U-238, and it can take the place of U-235. Plutonium is just as good as U-235 for carrying on a fission reaction.

The capture of neutrons by the U-238 in the structure adds on one unit of atomic weight and produces U-239. U-239 is an unstable isotope of uranium and it begins to break down radioactively and ejects two negative electrons and produces the new element, plutonium, Pu-239, an extremely valuable substance for our purposes.

You can look at this matter in another way, saying, "The U-238 is a poison in there for *a while*, but it gradually transforms itself into a useful substance to carry on the chain reaction. So, although the U-235 part is used up quickly, there is a delay period before you can use the U-238, that time being the period required for transforming itself into plutonium."

Now, plutonium is a new chemical element. It is element No. 94. It is artificially produced in this way. It may have existed in our universe at some early stage of our galaxy, but it has a half-life of only thousands of years and certainly disappeared from our cosmic picture long, long ago. We have re-created it! As somebody said, it is just like seeing a dinosaur walking around! You have in your laboratory and before you an extinct atomic species which has been brought to life. The metallurgists have all the thrill of investigating a metal which has never existed before — at least within the lifetime of the human race.

Since plutonium is a new chemical element, distinct from uranium, it has not the same chemical properties as uranium, so it can be chemically separated from uranium. Ordinary chemical operations, such as precipitation, solution, oxidation, reduction, can be used to recover the plutonium produced automatically in these piles. That is not true, of course, of the U-235 which is an isotope of uranium, and no *chemical* methods will separate it from the much more plentiful U-238. That is why the use of plutonium in the bomb development, besides being very ingenious, is practical. In order to get a pure atomic species subject to fission by slow neutrons, you change the uranium into another element first and then extract it chemically, thus avoiding the difficult separation of the uranium isotopes.

We now come to the rather sinister consequences of this business. I mentioned the desirability of getting rid of some of the U-238 to make the chain reaction go better and to leave proportionately more of the more reactive substance U-235. We now find that plutonium is fissionable, like U-235. What are some of the effects of this? If you take away the poison, the U-238, out of the system, the critical size of the pile starts to shrink, and smaller and smaller is the size necessary for a chain reaction. You need less and less graphite in the lattice, you need to slow down the neutrons less and less as you make the metal purer. Finally, as you get to the place where the stuff is truly pure, you don't need any graphite at all and the chain reaction goes with fantastically high speed! The time which it takes to multiply a generation of neutrons gets shorter and shorter until it is almost instantaneous, taking the fantastically low level of less than a millionth of a second. Under these circumstances you do not have a controllable chain reaction at all. You have a bomb!

Source of the Uranium

SMITH — Before we consider the bomb, I wonder if it wouldn't be a good thing to go back to some of the interesting things about the plutonium producing pile. It would be interesting for us to have Dr. Kinzel tell us something about the uranium metal that went into this pile.

KINZEL — The beginning is in the earth, or a hole in the earth, as you will. Uranium is a metal similar to many metals with which we deal. In fact, we had previously done a lot of experimenting with the metal uranium as an alloy in high speed steels and other alloys. We haven't found it particularly useful in that application. Even though uranium has long been known as constituting a small portion of the earth's crust (that, of course, is true of many elements), it doesn't do us much good when it is widely dispersed. We have to have the source concentrated to a point where it can be mined economically. There are two minerals which are most common for mining of uranium — carnotite, which also contains vanadium, and pitchblende, which is more familiar as being the source of radium.

The uranium ores are mined — in underground mines mostly for pitchblende, and open cuts or shallow workings for carnotite — and put through the usual concentration operations which do not differ in type and kind from those with which we are familiar for the other metallic minerals. After concentrating the ore to a certain degree, the concentrates are further purified in plants where the last bit of vanadium (in the case of carnotite) and other impure matter (in the case of pitchblende) are taken out. A pure chemical salt is the result. That salt is the starting point for reduction metallurgy and also it is the starting point for the separation of isotopes. As to reduction metallurgy, John, you had a lot to do with that; why don't you tell them about it?

Metallurgy of Uranium

CHIPMAN — Well, the reduction metallurgy of uranium has made some progress in the last few years. Back in '41 and '42 the present methods had reached the laboratory stage. Before the National Defense Council became vitally interested in the subject and before the large concentration of scientific effort was focused on the chain reaction, these methods had been evolved: A method for producing uranium from the chloride by reaction with calcium, a method that had yielded quantities measured in pounds rather than grams. There was also a commercial method for electrolysis of the fluoride that had also yielded a number of pounds. There was, further, a method for the direct reduction of the oxide with calcium or calcium hydride. One difficulty was that these last two methods produced metal powder, whereas lumps were needed. We did not know how to melt and cast these rather pyrophoric and very reactive powders, but developments in 1942 gave us a new method which yielded good lumps.

The people who investigated the structure of uranium by means of X-rays called it a pseudo-metal because it had a very peculiar structure, not encountered in other metals. It turned out to be, nevertheless, quite a ductile metal; it can be rolled or extruded and therefore put into desired form. It was learned quite early in the project that the quoted melting point (which you will find as 1800 or perhaps 1850° C. if you look in International Critical Tables or any other good reference book) fortunately was in error. The melting point is actually somewhat below 1150° C. or 2100° F. So, one can chalk up an error of some 600 or 700° for the standard reference books!

The metal occurs in three allotropic forms which we properly christened alpha, beta and gamma. Of course, each of these has its own peculiarities; one will roll or work better than another — as would be expected. Phenomena which are well known in other types of alloy systems are also encountered in uranium alloys. Several metals are soluble in uranium in the gamma form and one can quench these alloys and get results which remind us so much of the results we get when hardening steel that we sometimes refer to the gamma solid solutions as "austenite".

The metal possesses one characteristic which has caused a great deal of trouble: It is a thing which corrodes very rapidly. When you get power out of a piece of uranium, it is obvious that heat will be generated. If the structure is not to heat up too much for its own good, there must be some cooling medium. In one of the structures that was built the cooling medium was air, but we quickly found that air reacts with warm uranium quite rapidly. We, therefore, investigated quite a number of methods for coating or protecting our lumps of metal. I can remember one of these which involved some very complicated electroplating. The electroplaters did a very beautiful job of putting on several coats of various metals and the test pieces resisted warm air beautifully for three or four days, but on the fourth night three of them caught fire in the oven and the uranium vaporized inside the coating and left a hollow shell. So that method went out!

We finally protected uranium against warm air by sealing it up in an aluminum can. That same sort of process was used for protecting the metal against corrosion by water in the water-cooled piles. Water is also corrosive to uranium, particularly if the uranium is allowed to heat up as it most assuredly will if energy is evolved. Efficient protection was necessary and the development of this method of coating turned out to be one of the toughest of the problems before the metallurgists of the Chicago project.

SMITH — I think it is rather amusing to point out that when the University of Chicago got in on this chain reaction, the physicists thought they must have a nice blind so they could throw in a lot of activity and no one could guess what they were up to, so they labeled the project "The Metallurgical Laboratory". It wasn't long before a good many of the critical problems actually turned out to be metallurgical, and they had quite a few metallurgists on the job.

CHIPMAN — True enough, there was not one metallurgist there at the time the name was selected — all physicists!

Smith — Before turning away from the subject of natural uranium metal, it should be said that particularly good work was done by Spedding at Iowa State College, who not only developed the method, but produced practically all the metallic uranium used by the project for several months while the industrial manufacturing concerns were trying to get started.

Now, I wonder if we shouldn't say a little bit about how U-235 is prepared. Will you talk about that, Sam?

Allison — Well, I said something about plutonium. U-235 achieved fame even before the project started as a war measure because of some articles in *Colliers*, *Saturday Evening Post* and the *New York Times*, but as I hinted before, the preparation of pure U-235 is really a very formidable thing because it only exists as 0.7% by weight in natural uranium and it is chemically not separable from the other uranium isotopes. There is no chemical operation which will prepare it in a pure state. Purification, therefore, is really something to tackle!

The physical method for doing this thing was a straight electro-magnetic separation, and this depends upon the difference in mass between U-235 and U-238. You have a pretty small differential to work on there. So, in the physical method a beam of electrically charged ions was swung around in a circular path by a magnetic field. The particles that were heavier, the U-238, weren't so easy to curl into a sharp circle as the U-235. After they have traveled about 180° you will find U-238 on the outside of the average curve and U-235 on the inside. By putting in a little collection chamber on the inside of the beam you could catch the U-235.

Now, this is a beautiful scheme in principle and the idea was well known and had even been used to prove that certain substances, supposedly homogeneous, were actually mixtures of isotopes; however, no physicist in his right mind would have ever considered that any appreciable quantity of stuff could be separated that way, because the amount of material that you can get into an ion beam is infinitesimally small.

But this was war! Anything should be tried. A simply enormous plant, staggering to the imagination of a physicist, was set up with tons and tons of copper coiled about magnets, and miles and miles of vacuum tubes, and the thing was just "bulled" through until a ponderable amount of U-235 was separated.

Another method to be tried was the diffusion method where you use uranium in a gaseous compound, and take advantage of the fact that the molecules of the gas containing U-235 are a little livelier and get around a little faster than those containing U-238 — hence they will diffuse through a narrow opening a little faster. If you put the gas, stated by Smyth to be uranium hexafluoride, through a lot of little holes — a porous diaphragm — you will get more light uranium coming out the far end than went in. I want to sign off here because Mr. Kinzel knows much more about the details and he ought to tell you something.

Aerial View of Clinton Engineer Works, Oak Ridge, Tenn. (Courtesy Lincoln Electric Co.)

Smith — Anything conceived by a physicist sooner or later turns out to involve a metallurgical problem! Go ahead, Gus.

Kinzel — Well, this one certainly did! It turned out to be a metallurgical problem in more ways than one. I don't mean by this that there weren't a lot of other problems involved too, but in these separation steps you are dealing with corrosive materials. Accordingly the containers had to be carefully selected with due caution, and duly accounting for that corrodibility. You want to remember that the uranium material, by the time it gets to this stage, gets to be worth quite a lot. The efficiency of the operation also gets to be quite important; you can't afford to lose material by corrosion, for one thing. Also, the matter of leakage is something to think about. Brand new methods were developed for assemblying blowing equipment; there were thousands and thousands of blowers and pumps in these plants and thousands of miles of piping. In one plant we figured we could not tolerate leakage *in the whole plant* greater than would get out of one fine drill hole.

The order of magnitudes are of importance and are stupendous. The physicists did a wonderful job, and the industrialist and engineers did an equally wonderful job, to make a plant of this kind work. A number of new metals were designed from the standpoint of analysis; new stainless steels; new hard metals. Their number is more than a few. After they were so designed they had to be turned out in quantity, and they *were* turned out in quantity.

The electro-magnetic plant that was mentioned was in the same category, metallurgically, as the diffusion plant. In any other of the physicochemical methods that was tried, you had the same basic problem of avoiding *any* loss of material and in maintaining a constant, balanced flow of the materials while processing. The alloys used for handling and containing the materials must stand up under very peculiar conditions, differing from the ordinary combination of pressure, temperature, chemical activity, and so on.

Smith — I would like to talk a little bit from my own point of view. I was at the laboratory at Los Alamos, near Santa Fe, New Mexico, at what might be called the "shooting end". It was our job to take the materials made by other parts of the project and convert them eventually into the bomb. The reactant that was used in the bomb is in the metallic form. And so it is that explosives are no longer the sole province of the organic chemist — they are now also part of the metallurgist's job.

At Los Alamos we received neither plutonium nor the separated uranium U-235 in their final form. It was our job to do some purification, produce the metal and convert it into pieces of the necessary degree of purity, size and shape. Rather stringent purity requirements had to be met. Difficulties with U-235 were not enormous because it is metallurgically exactly the same as ordinary uranium (which, as you know, is mostly U-238). We therefore had plenty of opportunity to develop and practice the techniques by preliminary work with ordinary uranium, and were able to have well-tried metallurgical techniques ready before we handled the really valuable material.

Plutonium was quite a different story. We had to be ready to go into full-scale production the moment we had enough of it to work with. The Chicago laboratories had acquired a great deal of information working with just a few micrograms of plutonium salt. There the job was handled mainly by microchemists and they succeeded in finding out, before there was enough salt for clumsy-fingered metallurgists to work on, approximately what the melting point and density were, what would probably be a good reducing agent, its reactivity, malleability, and so on. At Los Alamos we didn't attempt anything properly called micrometallurgy, but we did a great deal of our development work with amounts of material that to a works metallurgist would be invisible — and that would be minute even to someone accustomed to extremely careful and precise work with precious metals.

Both metals are very reactive. There was a very difficult refractory problem. I have mentioned that we had to produce these metals to quite high purity specifications, yet they are quite reactive and most ordinary refractories react with them. One of the earliest projects was therefore to provide us with refractories which were considerably better than any then commercially available.

To make life more interesting at Los Alamos, plutonium (and to a less degree uranium-235) is a thoroughly unpleasant material to handle. It is radioactive and it is highly toxic by virtue of this radioactivity. We do not know how much it would take to kill a man; we believe the amount could be measured in micrograms rather than milligrams. A very, very small amount in the human system would doubtless produce a slow and unpleasant death. So we had to carry out all of these operations, not only on small quantities of material but under conditions where we would be sure that we would not breathe or ingest even traces of plutonium into our bodies.

There was one other hazard we had to be aware of: We knew if we got too much of this stuff together we would have a chain reaction going on before we wanted it, so we had to use very careful controls to see that we did not get at any one time in a given apparatus — or preferably in a given laboratory — more than the amount that could possibly be chain-reacting under any conditions that were likely to occur.

Kinzel — We played that one safe!

Smith — We did! We had to set up quite a system of guards to see that people did not unconsciously do things that they shouldn't. We kept track of every milligram of the stuff while we were working on it.

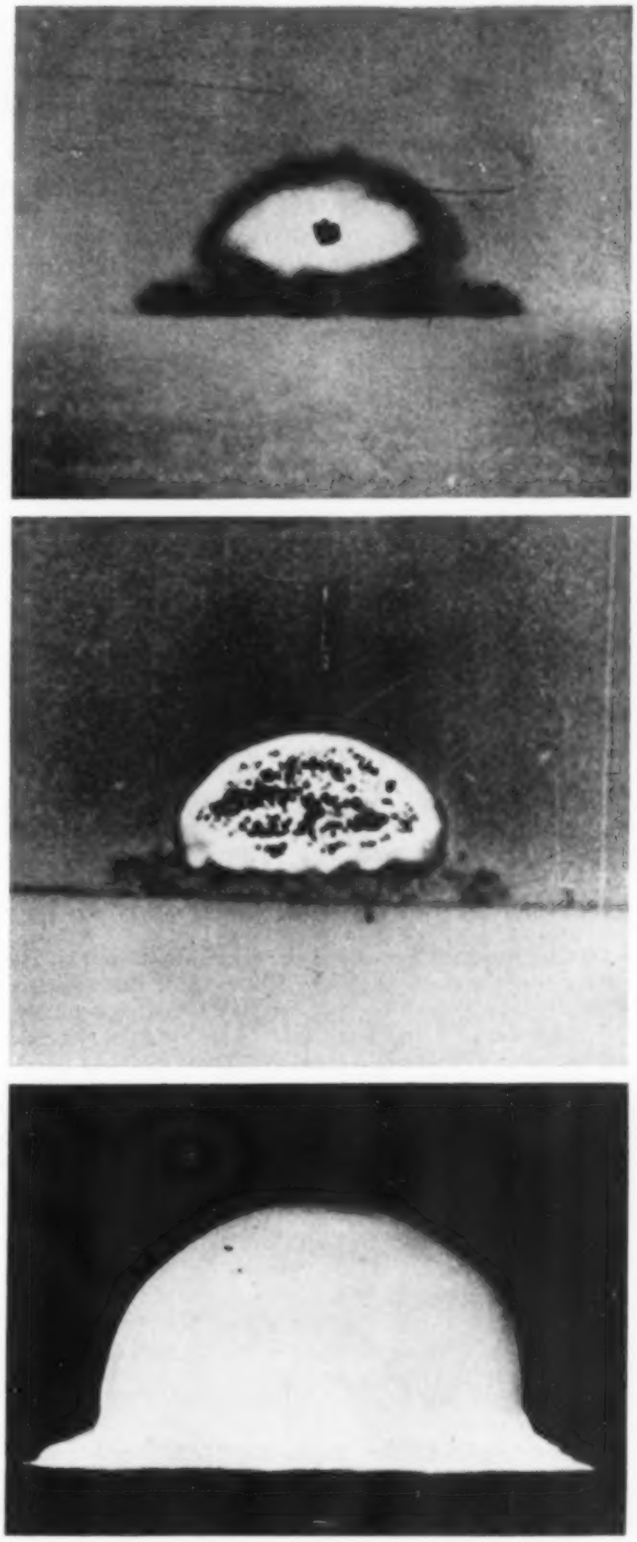
From my standpoint another thing made life at Los Alamos quite intriguing: The physicists, I found (with every respect to Sam Allison here) had active minds. They were constantly demanding of us materials of the most astounding properties! I sometimes suspected them of dreaming up things which were difficult to do just to keep us metallurgists busy!

Chipman — Cyril, may I remind you of a sign that used to hang in Sam Allison's office which said: "You Don't Have to Be Crazy to Work Here, But it Helps."

Smith — The culmination of all our work at Los Alamos was, of course, the bomb. I will never forget — no one who was there will ever forget — the few days at the base camp prior to the first trial. Probably most of you have read newspaper accounts which are really more colorful than anything I can say. I will never forget the night before the trial. All of the firing and recording equipment had been tested; the bomb had been assembled and mounted on its tower; everything was perfectly in readiness before nightfall, but we had to wait throughout the night — the trial, you know, was scheduled for 4 o'clock in the morning. It was a perfectly terrible night; it was raining hard and no one could sleep anyway. Around 3 o'clock we heard that the trial would be postponed a little bit and shortly afterward it was said that it would be at 5:30. Five-thirty came and the trial went off remarkably as predicted.

We thought we knew pretty much what was going to happen. The theoretical physicists (for whom I have come to have the greatest admiration) had been able to calculate how wide the area of destruction would be, what the shock wave would be like, and all that sort of thing. They weren't terribly far off in spite of the fact that no one had ever observed an explosion on anything like that scale. The thing which impressed everybody was the terrific flash of light — even though we knew roughly how bright it would be. We could see all the mountains around there light up with intensest brilliancy, much whiter and brighter than anything we had ever seen, even from arc welding or electrical discharge.

(Cont. on p. 772)



U.S. Army Photographs at 6-Mile Range of First Test of Atomic Bomb at Alamogordo, N. M. The top picture was taken immediately after the detonation. The middle picture shows the development of a multicolored cloud surrounded by a blue glow. Finally the tremendous cloud spreads out in the still air like a white derby. It later rose 40,000 ft. into the stratosphere

The Atomic Energy Act of 1946

A SPECIAL COMMITTEE has been appointed by the U. S. Senate "to make a full, complete, and continuing study and investigation with respect to problems relating to the development, use, and control of atomic energy". The chairman is Senator Brien McMahon of Connecticut; Edward U. Condon, head of the National Bureau of Sciences, is its scientific advisor. This committee started hearings Nov. 27, 1945, taking evidence principally from prominent scientists and members of the War and Navy Departments. The committee also visited the plant at Oak Ridge, Tenn., and inspected the operations there.

In the January 1946 issue of *Survey Graphic* Senator McMahon summarized the problems before his committee as follows:

Overall Problems — "Clearly, we must find ways to control the destructive power of atomic energy on a world level before we can make much progress on any concrete program to turn it into a blessing to mankind. Meanwhile, how far can we Americans ourselves go toward developing and using atomic power wisely; how can domestic legislation protect both our workers and citizens from hazardous developments; how control critical natural resources consistent with possible future international regulations; how protect both the individual and the nation? Consistent with world security, we cannot permit our domestic bill to stifle research; rather it must encourage our men of science to push back the frontiers of knowledge.

"The clock began to tick when the bomb was dropped at Hiroshima and it will stop when some other nation begins to produce its own bomb. Within that time interval we must do everything humanly possible to establish a feeling of confidence throughout the world and, at the same time, we must establish, through the United Nations Organization, a body of international law which will be effective. Therefore, our committee must help clarify three questions at the start:

"1. What is the expected time interval in which we may succeed in obtaining world security? We search for an answer in the light of experience from 1941 to 1945 in developing our own atomic energy program.

"2. Is there a scientifically feasible system of international inspection and control which can determine whether bombs are being manufactured by any nation? We should not be lulled into thinking in terms of past systems applicable to electricity, narcotics, and various contraband.

"3. Can we master the distinction between scientifically feasible and politically feasible acts?

"In establishing any body of international law through the United Nations Organization we must seek a new definition for that stage in the misbehavior pattern of any nation when it essentially declares war on other nations. The threshold for aggression would seem to lie very close to the point at which any nation begins to perturb the normal functions of a system of international inspection and control over atomic energy.

"Moreover, might it not well be the reciprocal duty and obligation of citizens of any and all nations to help maintain such effective controls? And should not the United Nations be able to prosecute an individual or a group, for violations of a nature which may lead to world insecurity, without placing sanctions upon the nation itself? An accusation placed against all the people of any country for acts committed by a minority will only increase the resistive attitude of the nation concerned."

S. 1717 — On Dec. 20, 1945, Senator McMahon's Special Committee on Atomic Energy introduced a bill into Congress "For the Development and Control of Atomic Energy, Serial No. S. 1717". A prime difference between this bill and others on the same subject pending before Congress was its establishment of a purely civilian control commission of five members, rather than a board part civilian, part military. Section 2 of S. 1717 provides that:

"*(b)* Members of the Commission shall be appointed by the President, by and with the advice and consent of the Senate, and shall serve at the pleasure of the President. In submitting nominations to the Senate, the President shall set forth the experience and qualifications of each person so nominated. Each member, except the Chairman, shall receive compensation at the rate of $15,000 per annum; the Chairman shall receive compensation at the rate of $20,000 per annum. No member of the Commission shall engage in any other business, vocation, or employment than that of serving as a member of the Commission.

"*(c)* The principal office of the Commission shall be in the District of Columbia, but the Commission may exercise any or all of its powers in any place. The Commission shall hold such meetings, conduct such hearings, and receive such reports as will enable it to meet its responsibilities for carrying out the purposes of this Act."

(Other provisions of the bill transfer all fissionable materials, bombs, and production plants to the Commission; declare that the Commission shall have sole ownership of all fissionable materials within the United States; direct it to conduct research and development work in the military application of atomic power; to promote by specified means private research into nuclear processes; to own all patents issuing, after compensating the patentees; to license proper use of fissionable materials by non-governmental organizations; to disseminate freely basic scientific information which "shall include, in addition to theoretical knowledge of nuclear and other physics, chemistry, biology, and therapy, all results capable of accomplishment, as distinguished from the processes or techniques of accomplishing them"; to establish a Board of Atomic Information which shall disseminate "related technical information with the utmost liberality as freely as may be consistent with the foreign and domestic policies established by the President"; and to make quarterly reports.)

Civilian Control — On Feb. 1, President Truman endorsed this bill in a letter to Senator McMahon, writing:

"A commission established by the Congress for the control of atomic energy should be composed exclusively of civilians. This should not be interpreted to disqualify former military personnel from membership, and is in accord with established American principles embodied in our statutes since 1870. I would prefer a three-man commission in lieu of a larger group which administrative experience has shown unwieldy. It is essential that the members of the commission be full-time Government employees.

"The Government must be the exclusive owner and producer of fissionable materials. (*Fissionable* materials are, of course, to be distinguished from *source* materials from which fissionable materials may be derived. By fissionable materials, I mean such as U-235, or plutonium, or any substance enriched in these beyond its natural state.) It follows that there should be no private patents in this field of exclusive Government activity.

"The disadvantages of Government monopoly are small compared to the danger of permitting anyone other than the Government to own or produce these crucial substances, the use of which affects the safety of the entire nation. The benefits of atomic energy are the heritage of the people; they should be distributed as widely as possible.

"Consistent with these principles it is essential that devices utilizing atomic energy be made fully available for private development through compulsory, non-exclusive licensing of private patents, and regulation of royalty fees to insure their reasonableness. These provisions will assure widespread distribution of the benefits of atomic energy while preserving the royalty incentive to maintain the interest of private enterprise.

"In my message of October 3rd, I wrote: 'Our science and industry owe their strength to the spirit of free inquiry and the spirit of free enterprise that characterize our country . . . . (This) is our best guaranty of maintaining the pre-eminence in science and industry upon which our national well-being depends.'

"Legislation in this field must assure genuine freedom to conduct independent research and must guarantee that controls over the dissemination of information will not stifle scientific progress.

"Each of the foregoing provisions for domestic control of atomic energy will contribute materially to the achievement of a safe, effective international arrangement making possible the ultimate use of atomic energy for exclusively peaceful and humanitarian ends. The Commission should be in a position to carry out at once any international agreements relating to inspection, control of the production of fissionable materials, dissemination of information, and similar areas of action.

"I feel that it is a matter of urgency that sound domestic legislation on atomic energy be enacted with utmost speed. Domestic and international issues of the first importance wait upon this action."

Military Review — On March 13, Senator Arthur H. Vandenburg of Michigan (a member of the Special Committee on Atomic Energy) introduced the following addition to S. 1717 which was adopted by committee vote, Chairman McMahon alone dissenting:

"There shall be a Military Liaison Board appointed by the President composed of representatives of the Departments of War and Navy, in such number as the President may determine. The Commission shall advise and consult with the Board on all atomic energy matters which the Board deems to relate to the common defense and security. The Board shall have full opportunity to acquaint itself with all matters before the Commission. The Board shall have authority to make written recommendations to the Commission from time to time as it may deem appropriate. If the Board at any time concludes that any action or proposed action of the Commission, or failure to act by the Commission, is inimical to the common defense and security, the Board may appeal such actions or proposed actions of the Commission to the President whose decision shall be final."

This amendment was immediately criticized as having the effect of putting the military in control of any activity of the Commission it could construe as relating to "defense and security".

In justification of his amendment, Senator Vandenburg offered this rebuttal:

"The action of the committee leaves total and final authority over every phase of atomic energy in the hands of civilians . . . . The military men would have no vote in the Commission itself. Its only power is to say 'stop, look and listen!' Thereupon the President's decision is final . . . . I shall welcome the blessed day when the United Nations can establish adequate international controls and inspection which will outlaw atomic bombs forever. But until that time arrives I do not intend to ignore our national security."

Senator McMahon, chairman of the Committee, said:

"I am pleased that the President has reiterated his support of the vital principle that civilians must control atomic energy. Of course the Commission should consult and advise with the military on military applications of atomic energy. And this is as far as any civilian commission should be required to go. The military is noted for its reactionary position in the field of scientific research and development. It is because I am concerned about our Nation's security, as well as the development for peaceful purposes of atomic energy, that I want civilians to control this force unhindered by the military."

Then too, the people around me were very quiet; there was almost no sign of any human reaction for what seemed a long time — in fact not until after the arrival of the shock-waves, a matter of 45 sec., and only then did the observers let loose their pent-up excitement.

It was on the way back to Los Alamos that I began to think something more about the meaning of what we had done. As to its immediate effects, we did know what this thing was going to add up to; we knew that there would be a great explosion; we knew that we were releasing greater amounts of energy than had ever been dealt with before by mankind; yet I, for one, hadn't realized until then just what it was that I had helped build. The thing that did come out of that first trial, and has been intensified since, is the realization on the part of all of the scientists that worked on the bomb that what other people do *with* science must also be the concern of scientists. We cannot turn this thing loose on the world to do with it as it will.

Sam, you were at Alamogordo too; in fact, you actually chanted the magic word that touched the thing off. I would like to hear what you have to say about the trial itself and also what you thought about later.

Allison — Well, as Cyril says, nobody who was there will ever forget the really amazing experience. I didn't have my main job down there, but I got myself a job, *(a)* because I wanted to see the show and *(b)* because I could be of some use. My job down there was to announce the times. You see, we had to decide fairly late, within the last hour or so, the final instant at which we would initiate the explosion. That decision couldn't be made precisely very far in advance, although it was fixed for a certain morning.

We had observers and experiments all over the desert there and up on the mountains. They were to observe the color of the light, the intensity at the light, the pressure wave in the air, and a lot of other things, and they had to know the precise instant at which the thing was going to be detonated. My job was in a dugout about six miles south of the place where the bomb was. It was heavily reinforced with timbers and covered with earth. In it was installed a public address system which reached some of the rather near observers, and also a rather small radio which reached people on the adjacent mountains. After Dr. Oppenheimer and Ken Bainbridge had their final conference and told me just when the thing was going to be set off, it was my job to announce the time and count the minutes and seconds over the two systems so that people would be able to get their apparatus ready and be ready to read at the precise moment.

I knew that I was going to miss something, hid as I was in this dugout. I did miss something. I actually did not see directly the initial flash of light. I was busily counting; I started out with "minus two minutes", "minus one minute", "minus 45 seconds", and kept on counting seconds over the radio system for people to hear. At the moment I said "Now!" the whole dugout (which was dark inside and had a small door facing exactly away from the bomb) lit up with a tremendous light. All this came from reflected light from the mountains and scattered light from the ground, yet it seemed as if a blinding arc light had been lit inside this hole. I knew there was going to be *some* light, but nobody imagined anything like it actually was.

It was estimated that the temperature of the material at the most intense moment of the explosion rose to over 100 million degrees.

Smith — What sort of thermocouple did you use to measure that, Sam?

Allison — I didn't use any thermocouple. It is a mathematical expression. As a "temperature" it is meaningless. It was hotter than what we estimate the interior of the sun to be. As a matter of fact, it is doubtful that anywhere in our galaxy anything has ever got that hot.

The radiation from such an object was something tremendous, of course. The focus of the reaction cooled so fast by radiation that this tremendous temperature only remained a very short interval of time — a few millionths of a second — but due to persistence of vision in the human eye, it appeared to remain much longer.

To resume: This awful light came in the door and I still had to go on counting positive times — at least for a few moments. I, being a scientist, decided that I would make at least one or two observations. I put my fingers against the supporting timbers of the dugout; I was going to feel if there was any earthquake shock connected with the thing. I felt absolutely nothing; there was no appreciable shock through the earth. Knowing the velocity of sound, I knew quite closely when the noise would arrive. It did arrive as scheduled. There was a tremendous whishing noise, a very large noise, but also the impression of a great mass motion of the air; volumes of air were rushing in, through and around the dugout.

Well, the agreement was that I had done my duty when the sound waves got there and I could run out and look, myself. I was getting pretty curious by that time to see what it looked like. So, I dashed out the door and looked down toward the place where the bomb had been located. I couldn't see any details for I was six miles away. I didn't need to see details! There was a simply tremendous cloud — an approximately spherical cloud — which by that time had risen a mile to a mile and a half above the ground. In this cloud were the most fantastic color effects. It is really a shame that we haven't good color pictures of that, because the cloud was simply boiling and boiling. It looked like a jumble of argon lamps, reds and browns. All around this enormous ball of air, which must have been some 4000 ft. in diameter at that time, there was a blue glow because of the gamma rays from the radioactivity within the cloud mass.

This whole business was rising — of course really rising very rapidly, but it was six miles away, and it seemed to rise rather majestically toward the stratosphere. There were some thin clouds rather high up — probably about two miles above the level of the desert — and as the hot mass of gas approached them they vaporized. The natural clouds disappeared, blue sky shown through — it was early dawn by that time — and it looked as if a way were opening for this mass of gas to rise up into the heavens. Observers in airplanes found that it went to some 45,000 ft. where it was taken by the stratosphere currents and dissipated.

Among us humans there were all sorts of reactions. One very prominent physicist was so astounded by the flash of light that he didn't hear the noise. The greatest noise in the world and he didn't notice it at all! He was the butt of jokes afterwards, because we were worried about him; when Gabriel blows his trumpet, he'll be left behind if he didn't hear a noise like that! The psychological effect of this light was quite overpowering. Many people were, of course, awe-stricken and they were muttering, "Good Lord, what would happen if a thing like that went off in New York or Chicago."

I now want to say just a few words, stepping aside from any scientific role I may have. I want to speak as a citizen of the United States and express my personal opinion as to what is likely going to happen and what should happen about this thing. These bombs can be made to work. Now that we have shown that they can be made, any country in the world can start to make them. The basic scientific facts are known; it merely takes a technological development to make them.

Suppose these bombs are used in a future war. What is the defense? As I see it, there is just as much defense against the bomb as there is against torpedoes or against ordinary bombs. When your defenses shoot down a plane that is carrying an atomic bomb toward your city before it reaches your city, you have defended yourself against *that* bomb. If you can somehow shoot down a rocket before it gets to your city, you have defended yourself against *that* rocket. If you can hit a torpedo before it hits your ship, you have defended yourself against *that* torpedo. But torpedoes are still sinking ships! Some bombers and some rockets will always get through!

It is the same with the atomic bomb. The point is that there is nothing inherent in the physics of the situation, no peculiar quirk of nature, that will allow a real, a perfect defense to be set up.

This situation emphasizes the fact, although it is just one example, that in our time war has simply got out of hand. It has become an impossibility to even think of war as a continuing state of mankind. We simply *have* to stop it!

We know that many attempts to stop war have been made in the past and have always failed — but we can't give up. This time it is absolutely necessary to succeed! The atomic bomb has simply pointed-up the situation and captured the popular imagination. Atomic bombs, stratosphere rockets, bacteriological warfare — all these blind horrible weapons mean that another war simply cannot be endured.

"Agreed," you will say, "but how can the particular use of the atomic bomb be stopped?" I think it is technically feasible to devise a system of inspection which, if sponsored by the United Nations Organization, would detect the manufacture of atomic bombs anywhere in the world. A licensing provision would allow the peaceful use of atomic power, but would detect the perversion of such use in the making of these dreadful things. It would be similar to the manufacture of alcohol, which is controlled in this country to a considerable extent. If you are manufacturing alcohol, you have to allow government inspectors to enter your plant. The inspection problem is very well known, and the leakage of ethyl alcohol into undesirable channels is rather small. The distribution of morphine, cocaine and dangerous drugs is a matter that we know requires control, and we know we can control it. Similarly, the distribution and manufacture of dangerous materials which make atomic bombs possible can be controlled. In my opinion it is technologically feasible. It will be very difficult, but possible.

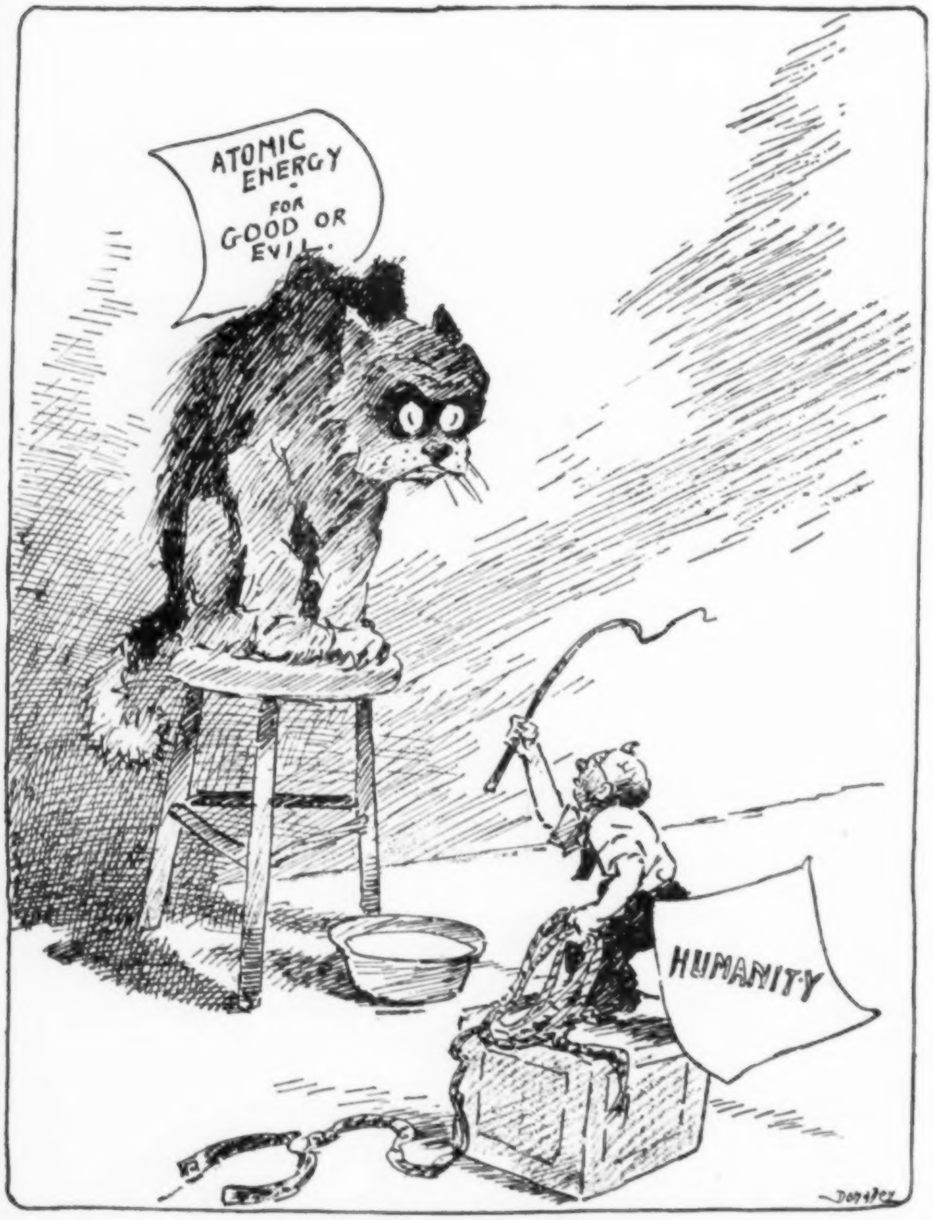
To do this, we will have to ask ourselves, "Are we willing as a country to give up some of our sovereignty and permit inspectors from the world government to go through our country, to go through our plants and report to the United Nations Organization that we, the United States, are not illegitimately using this new power?" I really believe it will involve some waiver of sovereignty, and any program for control will certainly be still-born if we ourselves are not ready to permit such free inspection by qualified inspectors. We will have to ask if we are willing that such inspection be made. We Americans are responsible for this thing; we made it. It is our job to take the lead in trying, with every ounce of power we have, to take away the fear from the world that such things are being made.

Very soon we will probably have a law establishing an atomic energy commission which will try to set up controls just within the United States. This commission must decide how far private industry can be allowed to go in developing atomic power without raising the suspicions of the government and the people of the United States that it is being perverted to anti-social ends. The controls we set up inside our country would be the model for some sort of world control. It is our duty to the world to devise a system of inspection, at first for ourselves, and then see if it can't be administered by a world government. Because this business cannot go on! We cannot live in fear of being blotted out by atomic bombs, or by bacteria, or by rockets. We have to make a great concerted effort to stop this thing once and for all.

Smith — I am very glad to hear you say that, Sam. It is my belief that what you have said represents the opinion of the majority of the men who worked on the project. Organizations of scientists were simultaneously formed at Los Alamos, Oak Ridge and Chicago and were the outcome of the concern felt by the men who worked on this thing that it should not result in the catastrophe which could come from its misuse. Many of us have since been branded as idealists, dreamers and so on, yet somehow I think we have shown ourselves practical enough during the war!

It is now time that we had some ideas about the future of this nuclear energy, particularly as to what it means industrially. I am going to ask Dr. Jeffries if he will talk about the future of nuclear *power* — what he thinks will come out of it in the reasonably near future.

Cartoon by Donahey in *The Cleveland Plain Dealer*


ATOMIC ENERGY - FOR GOOD OR EVIL
HUMANITY
DONAHEY

—And It Is Going to Take a Lot of Training!

Jeffries — It is difficult to talk exclusively about "power" in the sense of a prime mover because the release of atomic energy today is *heat* from a pile, and along with heat are three other things.

First is intense radiation. Today this energy is now mostly absorbed in shields, but it could be used for practical purposes.

Second, there are the fission products. When the uranium-235 or the plutonium atoms split in two they produce some twenty-odd other elements near the middle of the periodic sequence. Many of these are highly radioactive and, when separated from the uranium, use can be made of this characteristic for radiography, for "tracers", and for controls. In the immediate future their most obvious use is for a tremendous amount of research work.

Third, there is plutonium, whose industrial value cannot be appraised at present. The great plant out in Washington is now operated solely for producing plutonium for an explosive. In the longer future we could imagine that plutonium itself would always be a most valuable material. It might eventually be used to produce temperatures intermediate between these tremendous temperatures which Sam has mentioned and the more ordinary temperatures. It is also a remarkable way to store concentrated energy. No one living can accurately predict what uses might be made of plutonium in the future.

Now then comes the power itself. The heat can be produced at various temperature levels — at a small increment above room temperature, or at a very large increment. For the production of useful power in heat engines known at present, it is of course desirable to start with a considerable increment of temperature above that of the fluid at the end of the cycle. The use of a pile in connection with a heat engine introduces problems that have yet to be solved.

A few things we can say now will at least dispel some of the over-optimism which went around a short time after the bombs were dropped on Japan. There was talk about running automobiles with pea-sized engines, running airplanes all over the world on a grain of fuel, and so forth. Arthur Compton, who was director of the metallurgical project at Chicago University, answered these suggestions in a recent talk in Philadelphia. He pointed out that, for the present, the lowest weight of an atomic power plant that would be safe would weigh about 50 tons. That would rule out automobile and aircraft engines! Therefore, we should think about the larger power units.

Furthermore, in speaking about the larger units, we must think in terms of the power *plus* one or all of the other three by-products mentioned above. If the fission products, the radiation, and the plutonium were valuable enough, we could, for example, run plants primarily to produce them, locate the plants adjacent to big cities and use the heat energy as a by-product for heating the buildings. Or we could run them out in the country, a long way from coal or oil, and there the power would be very valuable because the transportation of ordinary fuel to such places would be very costly. It is conceivable that we could develop areas of the world that are now thinly inhabited or not inhabited at all. These power plants could be placed beyond the Arctic Circle or they could be run in the center of a great desert.

When we talk about the economics of power, we might keep in mind that the cost of the coal for making electricity is very little if the power plant is near a coal mine — probably not over 0.015¢ per kw-hr. In such a locality, the value of uranium for power (if we are thinking about power only) would be 0.015¢ per kw-hr. That is insignificant. The main cost of electrical energy in many instances is in the distribution, metering, and collecting the bills.

If one wants power to make metallic aluminum, he should be able to get it for somewhere in the neighborhood of 0.2¢ per kw-hr. If one wants power for industry, 1¢ a kw-hr. is a reasonable figure, and in the home, 3 to 6 or 7¢ per kw-hr. is acceptable. For starting automobiles we pay 50¢ or more per kw-hr. rather than hand-crank our cars. For running a flashlight, we pay $30.00 per kw-hr.! We may also expect, sometime in the future, that atomic power will fit into this price scale somewhere, because of some special condition, geographic or otherwise.

It may be that the by-products will be so valuable that the power could be thrown away and the piles still be operated. Those are matters which only the future can decide.

Smith — That is a very interesting point. I am sorry to keep calling on you, Sam, but since you are the only physicist in our group, I will have to ask you to explain a little bit about the by-products and why they are useful. Perhaps later on when you are finished, these metallurgists might give some ideas also.

Useful By-Products

Allison — In a sense plutonium is a by-product in itself, but I am not speaking of that right now. The fragments of the uranium atom after it has separated in fission are themselves radioactive and remain so for a long time. They keep splitting down more and more — not by fission but by emission of fast electrons, and they keep changing from element to element as time goes on. For example, one of the fragments might have the atomic number and therefore belong to the element xenon; it is a rare gas which soon emits an electron and becomes the metal barium. It stays barium for a while, then it emits an electron and becomes lanthanum. Lanthanum emits another electron and becomes cerium, and finally it becomes one of the stable rare earths.

All of these so-called fission products are present with enormous radioactivity along with the uranium in a running pile. They can be extracted. We then have in our hands very large quantities of radioactivity. This radioactive barium, for instance, is just one of many products, but it is quite an interesting one because it emits a very penetrating gamma radiation, and it could be used for a source in radiography. The concentrations which one can get are tremendous — the equivalent of thousands of Curies of radium into a mere speck of this active barium. You can carry it around at the end of a stick — a pretty long stick if you don't want to injure yourself from the radiations and use it like radium.

Smith — If you carried radium with you would it not do the same thing?

Allison — It would. Actually the radiations from the barium are not quite as penetrating as the radium radiations, but you could make up for that fairly well by having a more intense source.

That is just one fission product. There are others; there is strontium which is radioactive and which has already been used therapeutically. If you have a bone tumor (a very nasty sort of thing to have) and take some of this radioactive strontium in your system, the active metabolism in the bone attracts this strontium which is chemically similar to the barium and calcium of the natural bone, and it irradiates the tumor in the bone without affecting much of the surrounding tissue. It is far too soon to say that the results are certain, but it was an interesting way to treat bone tumor — by local radiation and not by X-rays going through the whole structure of flesh surrounding the bone where the tumor is.

In many fission products there are some radioactive iodines that can be used to investigate the action of iodine in the glands of the body and as a tracer in iodine chemistry. There is another entire class of radioactive substances that can be obtained as a by-product. If you stick almost anything into the chain reactive structure the neutrons that are floating around in there — and this presumes that you have some to spare — will attack this foreign substance and that substance becomes radioactive. There are only a few exceptions — off-hand I can think of helium; it does not get radioactive when put in a pile. Neither will hydrogen.

Chipman — Sam, would you put beryllium in the same class?

Allison — Well, you can make beryllium radioactive, although its radioactivity is very weak. It is a good example of something that almost doesn't get radioactive. It is a borderline example, for it is really hard to think of elements that are not made radioactive by being put into the pile. What really ought to be done — and this has been suggested by many people — is for the people controlling this thing (the Army, at present) to issue a catalogue of all the radioactive things that have been made. None of us — with all the concentration of brains at this table — can think of all of the possible applications. If such a list were made and distributed to technologists all over the country, I am sure that we could get many very interesting ideas of what could be done with them in the way of "tracers" to check on useful technological processes. One rather interesting quirk is that a given manufacturer of a metal could always tag his metal by putting in an innocuous quantity of one of those elements, so he could detect any substitution at any time.

Smith — There might be times when he wouldn't want to!

Allison — Well, I will leave that to a commercial metallurgist. But, for example, suppose the people that are making a particular kind of brass — this is a dark chapter in Cyril's past when I speak of brass — think they have a better brass than any other brass company. They could make that brass radioactive with a certain half-life period and with certain other characteristics by putting some specially treated copper into it. Then if they got a complaint from somebody using brass strip (it does not form well and is wearing out the dies too fast) and they suspected the customer is using brass from some other company, one of the home team could go out there, take a detector and say, "Aha, this isn't my brass. I don't care what you say!"

Smith — I think you ought leave things like that to the metallurgists, Sam. I would like to have discussed briefly the other types of isotopes which are very useful in research. After all, all of your isotopes are not radioactive.

Kinzel — Let me have a word before you go ahead on that, Sam. When Cyril asked about the analogy between barium and radium, you pointed out that indeed there was quite a similarity in their use for radiography. I think the important thing there, and one which we are apt to forget because we are dealing with it, is the *quantity* which would be available. The quantities of isotopes made artificially are so much greater than anything we have ever had in the past. You couldn't have a gram of radium in every metal manufacturing plant, but you could have one of these barium gadgets of yours.

Allison — Yes, the quantities would be enormous compared to what we have known before. They are measured in thousands of Curies of activities instead of units of Curies we have been using in the past. Those things would be available.

Now, Cyril told me to talk about some other things which may or may not be radioactive. I am beginning to get my memory back, and there are a few elements — and quite important ones — for which radioactive tracers cannot be made. Oxygen, which is an element of extreme importance, unfortunately has no usable radioactive modification that can be used as a tracer. That is also true, essentially, I believe, of nitrogen and that is also an important element in all sorts of processes. Useful possibilities are not there because, although we can make some very short-lived forms of those elements, their life is much too short for use.

Smith — I might mention the use of carbon and hydrogen isotopes in biological research. These can be followed by analysis with the mass spectrograph. However, where radioactive tracers are available they are perhaps easier to follow, particularly in very great dilutions.

I think we ought to mention the probable use of these tracers in metallurgical work, since those present in the audience are metallurgists, for the most part.

One of the earliest uses of radioactive metal was in the determination of diffusion constants. For example, if you want to follow the diffusion of copper either into copper or into zinc, or into any other metals, you would plate some radioactive copper on the surface of the specimen of stable metal, anneal it for various times, and follow the penetration of the plated metal by very simple counting technique.

There are a number of possibilities of auto-microradiography. Introduce a small amount of some radioactive material in an alloy — forge it, anneal it, heat treat it, do any of the ordinary fabrication operations — then take a cross-section, lay it on a photographic plate for a certain length of time, and you get a directly printed macrograph showing plainly the distribution of the radioactive material.

Could you, John, mention some possible uses in steelmaking?

Chipman — I have used tracers in metallurgical research even before they were available from such sources as uranium piles. We have had some of them from the cyclotron for quite a little while. Radio-phosphorus is one of the common ones which has proved quite useful. We used it on one occasion to study the equilibrium distribution of phosphorus between liquid steel and the slag in contact with the steel. We could determine very nicely the time at which the reaction reached completion, simply by using a Geiger counter on a sample of the steel. Things of that sort might be very useful in the steel industry as short-cuts for chemical analysis. Moreover, things can be done with tracers that cannot be done at all by chemistry. For the study of segregation in steel ingots, one can locate the macro-segregation — the segregation on a large scale, from inch to inch, say — by ordinary chemical analysis, but when one wishes to study micro-segregation, then chemical analysis becomes extremely difficult, if not impracticable. If we add a radioactive substance like phosphorus to the steel, it will locate itself quite accurately on a photographic plate. This technique, I am sure, will go far toward explaining some of the peculiar things that happen during the solidification of a steel ingot.

Smith — It seems to me that there is a chance here to find something about the micro-segregation. These methods could also be very useful in studying corrosion because it is possible to tell whether a given surface film on a piece of metal contains atoms which came from the environment or if they were in the metal to begin with.

Kinzel — You could use other phenomena, too. For example, in the case of boron, for which we don't have a convenient radioactive tracer —

Smith — That is a bad example because there doesn't happen to be a boron which is appropriately radioactive.

Kinzel — That is why I pointed it out, because it doesn't happen to be one that is. But you could go to work on steel to find out where the most neutrons were being absorbed and in that way locate the boron. In other words, I am pointing out that other properties of the boron having something to do with the nuclear reaction are likewise important and can be put to use.

Jeffries — I would just like to warn many of the people here, especially some of the younger ones, that in the longer future they will have to cope with the subject of radioactive *standards* in metallurgy. As radioactive materials become more prevalent, they will get into the materials of commerce, and then into scrap, and there will be specifications to meet. For example, certain materials for the photographic industry will have to have a certain minimum Geiger count or the customer will not accept them. One can imagine scrap going over a belt and being automatically Geiger-counted; anything high goes off this way, and anything low goes off that way. In any event, we must be prepared for new specifications and new controls.

Obviously people dealing with photographic materials can't have material around that is too radioactive because of the hazard of fogging a film. On the other hand, other people will want specified amounts of radioactivity; some of them will specify limits for rate of decay and initial intensity.

Nucleonics is going to add some very interesting new elements to the metallurgical world, not next year or the next after that, but it is coming. We may be looking back on the "good old days" when we only had to worry about 92 elements.

Smith — Do you think, Zay, that there is any danger at all of getting so much radioactive material in the world that there will be a health hazard when you buy a ton of steel?

Jeffries — I wouldn't say that there is no danger at all, but I think that is highly improbable. In any event, that will be part of the control. By the time radioactive material becomes sufficiently prevalent for that, the metallurgists will have been watching it for many years, and it would be a terrible metallurgical *faux pas* if one should make and ship such a metal as that.

Smith — It should also be mentioned that the amount of these tracers you have to use in order to get proper readings is extremely minute and very much less than the amount that is a health hazard. You will start off with relatively small amounts of these things in concentrated form, and you will have to watch them from then on.

Kinzel — Very much less than the amount of boron we now work with in steel.

Smith — Time seems to be getting on and I would like to leave time for a few questions from the audience, but I would like to hear Mr. Kinzel discuss the engineering and metallurgical aspects of power production. Although we have been talking of power, so far we have only got something hot; we haven't got any power out of it.

New High Temperature Metallurgy

Kinzel — I can go into it only briefly. When we have heat and we want to get some mechanical work from it, it must go through an engine of some kind. The characteristics of practically all engines we know are such that efficiency comes with high temperatures. We can suppose that when and if we get some power generated from piles, we are going to have high temperature problems and corrosion problems of the same order of magnitude if not greater than we have in present day practice — even with the gas turbines and the high temperature steam turbines discussed on this platform last night. These problems, unfortunately, are going to be even more complex because in these piles we are going to have materials that not only have to meet all the usual requirements — with which we are now more or less familiar — but in addition the neutron capture characteristics have to be right. Otherwise the pile will not operate properly, or the metal itself that is used may disintegrate. I can see a whole new field of metallurgy in which in the putting together of alloys one of the main limitations will be the "neutron capture cross-section". We are going to have to match everything else with that. I can also imagine working with special isotopes of some of the more well-known metals in order to make such alloys.

Anyone who thinks that there is nothing more to do in tonnage metallurgy (I heard that stated a while ago) certainly has a nice place to start.

Smith — I think it is now time we let people from the audience fire a few questions at us.

Question — How about the maintenance of atomic energy plants? Do they become auto-intoxicated, so to speak?

Allison — That depends, of course, on how hard you run the plant. I don't think anybody really knows the complete answer to that question yet. The plants at Hanford, as far as I know, are still operating and they have been operating considerably over a year, and they haven't clogged up yet.

Question — How often do you have to renew the raw material?

Allison — Well, that is a question that we would very much like to know the answer to because — it is a very intelligent question, too — it sets a limit on how long the plant will run. As much as I can say now is that we do not yet have a good figure on that, but they run a very, very long time. I can't say how long.

Question — Does the radioactivity described by observers of the Alamogordo test persist in the surrounding territory and endanger life?

Smith — It is very important that we distinguish between the test bomb and the two bombs exploded in Japan. You must all have seen in the papers the statement, which is absolutely true, that both combat bombs were purposely detonated at quite a high altitude in the air and there has *not* been detected any radioactivity on the ground in either of the Japanese cities. There were nevertheless many damaging physiological effects from the use of the bomb, even as we used it, on those who were so unlucky as to be within the radius of action. There was an effect due to instantaneous radiation at the time of the explosion. If you over-expose yourself near an X-ray tube when it is working, it has also very unpleasant after-effects — fatal, in extreme exposures. Conditions were different at the New Mexico site. There was residual radioactivity of the bomb crater in the desert; in fact it still is slightly "hot" there. However, at Alamogordo the test bomb was detonated comparatively close to the surface of the earth, and it was impossible to avoid mixing the products of the reaction into the ground. In combat use, all the products rose with the ball of fire high into the air and were dispersed by the wind currents.

Question — What is the chance of a chain reaction spreading to the surrounding air — an explosion without limit?

Smith — That is clearly one for Mr. Allison.

Allison — I have been very much disturbed by statements that such a thing is possible because they are quite untrue, and they are apt to be very damaging to people like myself who are interested in doing further research in atomic energy and in nuclear physics. I do not know of any way to start a chain reaction which will spread beyond the prepared material. Some people seem to believe that only part of the energy in the bomb came from plutonium and other parts came from the atmosphere around it, and that the nuclear explosion, somehow, spread out beyond the bomb. That is absolutely untrue. There is no way that I know or even dream by which the explosion could spread out beyond the active material itself. The reason I am so worried about this is that people may get to believe that scientists are such irresponsible men that they would make an experiment like that when there is the slightest possibility of the thing spreading out in the atmosphere. If they were of that sort, scientists ought to be locked up somewhere, and I don't want to be locked up. I want to be free to improve my own researches. Therefore, I am very eager to stop such rumors.

There was another damaging statement that was made in one periodical — namely, that an important physicist at the New Mexico test bet 10-to-1 that the explosion would spread beyond the bomb. That is pure fabrication. I talked to the man — he is a friend of mine and I was with him during the test — and no such bet was made at all. My friend was horrified when it appeared in print, he said if anyone had permitted this experiment to be made who thought there was a 1-to-1000, not 10-to-1, chance that a major catastrophe would take place, he ought to be shot.

Smith — Perhaps, Sam, one thing that is in the back of people's minds is some idea about what is going on in the sun. They suspect that the sun's energy comes from a nuclear chain reaction. Could you tell briefly what the present view is, and point out particularly why we can't have something like that down here.

Sun's Energy Not From Fission

Allison — As a physicist I will have to point out that it is not a chain reaction that takes place in the sun, in the sense that we have been talking about chain reactions this evening. It is what we call "thermo-nuclear reactions" wherein the temperatures and pressures are so enormous that the thermal energy with which the atoms vibrate in the hot mass is sufficient to cause the nuclei to collide and nuclear reaction to result. Now those temperatures and pressures are such that it is quite impossible for us to reproduce them in a mass of gas on the earth; we don't know how to compress hydrogen to millions of atmospheres and simultaneously to heat it to some 20 million degrees. If any metallurgist here can tell me of a vessel in which I can hold hydrogen under such conditions, I will begin to get worried. Until that becomes feasible, I am going to rest easy.

Jeffries — You could also add, I think, that this generation of heat takes place in the interior of the sun, not on the surface.

Question — Is it possible to operate a pile with molten uranium at a high temperature?

Jeffries — Yes; physically possible; but no one knows yet how to do it.

Smith — The refractory problem would become paramount if you would operate in that way for any length of time.

Kinzel — And a corrosion problem!

Question — In any system of international control, how is it possible to keep track of all the men who are competent to solve questions relating to atomic energy?

Smith — I think it might be possible to do so.

Jeffries — May I offer comment on that one, too? This is a matter relating to a combination of things. First, the great mass of scientists are honest and constructive. Second, their brains alone won't produce any harm. It takes brains, bad intent and a great amount of energy, all combined, to do the harm. The chances that those things will all combine in a society of good, constructive people are not very great.

Question — Can we have some ideas about the use of atomic bombs on a fleet at sea?

Allison — Some tests are going to be made. I have no official connection with the project any more, and so I can only make a Yankee guess. If the bomb is detonated at some distance above the sea, the pressure waves from the bomb will strike the surface of the ocean and will depress it. This will cause a series of waves in the ocean until the depression fills up. Heat from the bomb will vaporize a considerable amount of water and probably blanket the area with steam. The pressure wave would be very severe on objects near the explosion and anything that isn't anchored securely is going to be twisted and blown away. All these actions will be progressively less as the radius is larger. I think that the battleship structure may survive fairly well, but I wouldn't want to be in one of those battleships or anywhere near that thing, even if I wasn't afraid of the pressure wave, because if the ventilators are going and suck into the ship some of the radioactive gases and hot air, everybody in the ship is going to be in a pretty miserable state. Now, I have no inside dope, as I said; I am disconnected from the project. I am just guessing like an intelligent layman, so that is the interpretation you should put on those remarks.

Chairman Smith — The time allotted being up, the discussion is adjourned.

A.I.M.E.

Notes on the 1946 Meeting of the Mining Engineers

By Our Special Reporter

AT THE FIRST postwar meeting of the American Institute of Mining and Metallurgical Engineers, held in Chicago during the week of Feb. 25, a number of interesting papers were presented. Little of the information was a direct result of war research — most of it, indeed, was an outgrowth of work initiated prior to the war. It was also noted that the papers presented indicate a trend toward a more scientific approach to metallurgical problems. This trend, already noted at the recent National Metal Congress of the ⊕ in Cleveland, is welcomed. Metallurgists are now beginning to utilize the methods of physics and chemistry in the solution of their problems. The metal artisan should not be displaced by this new scientific worker, but the artisan should call upon him for assistance in modifying his methods. New tools for research are also being utilized and applied by physical metallurgists; electron microscopy and electron diffraction methods, such as those outlined by Dr. Gulbransen in the last issue of *Metal Progress*, are now being put to use in solving metallurgical problems.

One of the first applications of such electronic studies to investigations of the behavior of metals was reported by researchers of the Dow Chemical Co. Much attention has been given to electron microscopy at the Dow laboratory, and workers there have contributed to important developments of the technique. In the past, most metallurgical research with this technique has been exploratory; the results have not been directly related to other physical metallurgical studies. In a paper discussing electron metallurgical methods and their application to magnesium alloys, Messrs. Heidenreich, Gerould and McNulty establish a correlation between a fine structure so discovered and the stress-corrosion rate. Magnesium-aluminum alloys were studied and the fine structure is believed to be a segregation of FeAl. The amount of iron alloys in which the fine structure is observed varied between only 0.001 and 0.021%. Metallographs taken with light rays — the usual optical system — failed to distinguish between alloys having widely different stress-corrosion rates, while electron metallographs were directly correlatable with the rate of stress-corrosion.

Considerable discussion was heard at the meetings of other important applications of this technique to physical metallurgy problems. Workers at the Dow Chemical Co.'s laboratory, in papers printed recently in several periodicals, have reported the discovery of a precipitate of iron nitride which occurs during the initial tempering of martensite. Studies of the relation of this precipitate and the properties of tempered steels may well be one of the next applications of electron diffraction in metallurgical techniques.

Decomposition of austenite received its share of attention at the A.I.M.E. meetings in discussions reminiscent of the ⊕ panel in Cleveland. Some of the problems presented by the Cleveland panel were attacked in papers read in Chicago before the A.I.M.E.'s Iron and Steel Division. The relation between the decomposition of austenite upon continuous cooling (quenching) and the changes in austenite at constant sub-critical temperatures (austempering) was the subject of two papers. In order to apply the S-curves derived from austempering experiments to practical heat treating problems, it is necessary to understand the kinetic relations between constant temperature transformation and cooling transformation. For example, the information obtained from S-curve studies should be directly related to information gained from Jominy hardenability tests on the same steel, but as the author of one of the papers stated, "no strong connective link between the two methods of study has yet been developed". The establishment of this connecting link is one of the most practical and theoretically interesting problems in the physical metallurgy of steel. It will involve an understanding of the mechanism of nucleation, and this is a subject of immense complexity. A direct experimental attack is favored by some metallurgists, although the quantitative study of reactions occurring during a matter of seconds is certainly not easy.

A means of deriving the mode of decomposition of continuous cooling from isothermal data for the pearlite and ferrite transformations was suggested many years ago. Confirmation of this method was presented in both of the new papers, one by Manning and Lorig of Battelle Memorial Institute, and the other by Hollomon, Jaffe and Norton of Watertown Arsenal. However, the information regarding the bainite reaction presented by the two groups of authors was in conflict. In one paper, results indicated that nucleation by pro-eutectoid ferrite would accelerate the formation of bainite, while the authors of the other paper indicated that they had found no such effect. This entire problem has been investigated only sketchily and needs a great deal of attention.

The martensite reaction is one that can only be studied on a continuous cooling and was the subject of a new paper by Grange and Stewart of the U. S. Steel Corp.'s research laboratory at Kearny, N. J. New data for the effects of alloying elements on the reaction were obtained in this study, and a method (similar to that originally discussed by Payson) of approximating M_s, the martensite start temperature, by computation was included. Following the presentation of the paper there was held a philosophic discussion about methods of physical chemistry and thermodynamics which can be applied to the problem. Predictions of the new theory of Clarence Zener* regarding martensite formation are generally in accord with the new data. There are some exceptions to the predictions of the theory and the discussion concerned whether or not these exceptions invalidate the theoretical method or whether they simply indicate where the theory must be modified.

New data were presented by Hodge and Orehoski of Carnegie-Illinois Steel Corp. on the effects of alloying elements on hardenability. These authors studied the effects of alloying elements in steels in which bainite rather than pearlite limited the formation of martensite. They found factors relating alloy content to hardenability much in disagreement with the original Grossmann factors but nearly in accord with those recently obtained at the Naval Research Laboratory, and described generally, if briefly, in the report of the ⊕ convention in *Metal Progress* last month. There is so much disagreement still existing as to the effects of alloying elements that this paper can only be considered as preliminary to the work necessary toward establishing an accurate system for computing hardenability from chemical composition.

In a session on the physical chemistry of steelmaking, several papers on the equilibrium between liquid iron and slags caused much discussion. One of these, by Winkler and Chipman, relating to the distribution of phosphorus during melting and refining, involved the use of radioactive tracers of phosphorus. This technique has immense possibilities in the study of equilibrium in steels and in fact for the study of innumerable chemical reactions. The recent achievements of nuclear physics provide a host of new radioactive materials for metallurgical studies. Obviously the rates of diffusion of many elements and the rates of many reactions can conveniently be investigated by using tracers — likewise the eventual location of many substances held in alloys in such minute traces that their identification otherwise has so far been impossible.

A diffusion study involving isotopes of nickel was the subject of another paper by Wm. A. Johnson of Westinghouse Research Laboratories. For most work, it is not necessary to consider the difference in mass of the various isotopes of the diffusion element. However, in attempting to obtain a mechanism for solid diffusion, the effects of varying mass must be understood. As in the case of gases it was found in the present study that the isotopes of the smallest mass of nickel diffused fastest through copper. (A mass spectroscope was used to determine the isotopic concentration of nickel in the copper.)

A discussion of diffusion of a type was also presented in a paper on "Anelasticity" (elastic after-effect, primary creep) by Clarence Zener of University of Chicago's newly established Institute for the Study of Metals. Zener pointed out how the diffusion of carbon and nitrogen atoms from one place to another in the iron lattice could contribute to damping or cause primary creep. Because of the effects of these elements, he proposed that the rates of diffusion of carbon and

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*"Kinetics of the Decomposition of Austenite", A.I.M.E. Technical Publication No. 1925, in *Metals Technology*, January 1946.

nitrogen in ferrite could be studied by determining the time-temperature relations for the damping of elastic vibrations in steel.

Other causes of elastic after-effect and primary creep were also discussed in the new paper. Old ideas of Rosenhain concerning the viscosity of grain boundaries were reviewed. New experiments on the behavior of metals at high temperatures seem to demand that grain boundaries behave in a viscous manner, and that there is in reality a range of temperature in which there is equal cohesion between the grain boundary material and the material of the grains themselves. The half-forgotten — even discarded — ideas of the past about amorphous metal at grain boundaries and equi-cohesive temperatures seem to have new application to metal behavior!

An important advance in permanent magnet metals was also reported by E. A. Nesbitt of Bell Telephone Laboratories. Alnico, the strongest permanent magnet material, is unfortunately not easily fabricated and cannot be rolled into sheet or drawn into wire. A newly developed metal called "Vicalloy", consisting of iron, cobalt and vanadium, is not only capable of such fabrication, but its permanent magnetic qualities improve with cold deformation. Vicalloy is not as strongly magnetic as is Alnico, but has good enough properties so that its ability to be fabricated will make it useful in many forms and in many places where Alnico cannot be easily put.

At the powder metallurgy symposium a new trend was evident in the discussion of the problems of pressing, sintering, and the properties of metal powders. Heretofore there has been considerable hesitancy by the industry to disclose technical information about the processing of powder compacts. Speakers at this symposium, however, discussed freely some of their important technical and engineering problems. Some of the properties possible by hot pressing in controlled atmospheres were discussed by Henry and Cordiano, and effects of small amounts of alloy additions to copper compacts were also reported by Hensel, Larsen and Swazy. A new method of studying the stresses and pressure variations during compacting was the object of a great deal of interest, and results already available indicate that the new method will be extremely helpful in elucidating the effects of lubricants. The sintering process was also subjected to a critical review by Wulff, Seelig, Delisle and Finger, and a research program was outlined that it is hoped will resolve many of the conflicting points of view, as well as clarify the sintering mechanism.

Numerous honors were bestowed at the banquet. The John Fritz medal, established many years ago by the so-called Founder Societies, was awarded to Zay Jeffries, vice-president of General Electric Co. and past-president ⊛ "for leadership in the solution of problems affecting the production, conservation, substitution, and the scientific appraisal of metals and alloys". The Institute of Metals Division Award for 1946 was presented to P. W. Bakarian and C. H. Mathewson for their paper entitled "Slip and Twinning in Magnesium Single Crystals at Elevated Temperatures". The J. E. Johnson, Jr., Award was made to John J. Alexander "for developing a method for connecting two turboblowers in series in order to blow 100,000 cu.ft. of wind per min. at high pressure, and for his work on adding a controlled amount of moisture to the blast". The Rossiter W. Raymond Memorial Award was made to Capt. John H. Hollomon for his paper on "Tensile Deformation". T. L. Joseph, professor of metallurgy and assistant dean, Minnesota School of Mines and Metallurgy, gave the 23rd Howe Lecture on "The Blast Furnace Process and Means of Control".

At the annual banquet at the Palmer House, a brief and non-technical talk of extreme importance was made by Alan E. Cameron, president of the Canadian Institute of Mining and Metallurgy. In recent years the problem of union activity and labor organization of technical people has been the subject of much controversy. Mr. Cameron pointed out that the Canadian organization of which he is president has recently appointed committees to study the entire problem, and he encouraged and invited the A.I.M.E. to take similar steps. He emphasized a point often overlooked in such discussions: It was not a question of organization of technical people to obtain new rights for themselves, but of organization to prevent non-technical people from acting as sole bargaining agents for professional engineers and scientists. In Canada, as in America, a single union is often delegated as the sole bargaining agent for a particular company or a particular industry. If it is ruled that some union of non-professional workers can act as bargaining agent for professional people, then the present comfortable relations between professional employees and management may be disrupted. Mr. Cameron pleaded for attention to this problem so that minority interests of mining engineers and metallurgists could be protected. With the present trend in labor relations in both Canada and the United States, this non-technical problem may actually be of more importance to scientific advance than any of the recent papers discussed at the A.I.M.E. meetings. ⊛

Correspondence

Russia Plans for the Future

SAUK CENTER, ILL.

To the Readers of METAL PROGRESS:

Much information about wartime industrial and scientific developments, gained by recent American observers in Germany (that is, the portion occupied by our troops), is being published or is available in official documents. However, corresponding information about what is going on within the confines of our large ally Russia is so scarce that I would call your attention to some observations made by Irving Langmuir, General Electric Co.'s eminent physicist, during his visit there in June of 1945 to attend the celebration of the 220th anniversary of the founding of the Academy of Sciences in Moscow. This material is contained in his testimony before the Senate Committee on Atomic Energy. He testified that Russian scientists spoke far more freely than the American and English were allowed to speak, and that visitors had excellent opportunities to observe Russian scientists at work and to learn of the extensive science program that is being planned (a program that is one of the two major objectives of the next 5-year plan announced recently by Premier Stalin).

Langmuir notes that responsible governmental officials in both England and Russia have announced plans to produce the substances that will give them atomic power — also bombs. He also agrees with other experts that there are no "secrets" in this development we now have that cannot be discovered by other researchers and engineers. He warns against the tendency to discount the Russian abilities to match our achievements, saying that they long ago held the record for long range airplane flight, that the Germans captured or destroyed about ten times as many Russian-made farm tractors as the total number existing in all Germany, and that during the war "they devised and built in quantity tanks as good as the best made in Germany. They are now engaged in the construction of a large experimental plant, of the order of $100,000,000, to operate a blast furnace with oxygen instead of with air", and are making plans to convert their entire iron industry if the first venture is the success they anticipate.

Most of the work done in the Russian scientific institutes during the war was of a long-range character that would find application only after the war. The Russians deferred men from military service for such work — something the United States did not do. Russia has 790 universities, and they were training more men during the war years than ever before.

"Some of the older laboratories had equipment much like that of German laboratories of 20 years ago, but some were splendidly equipped and had very skillful staffs. Several large new laboratories were nearing completion, and these are to have excellent equipment. I was shown plans for a new building for the Academy of Sciences about five to ten times larger than the present one."

For the above reasons Mr. Langmuir is of the opinion that Russia can produce atomic bombs without undue delay. In any armament race he believes they can outstrip us because "the following factors give them certain advantages in such a race:

"1. They have a larger population, and it can be regimented and is willing and has been trained to sacrifice living standards for an extensive defense program.

"2. They have a remarkable system of incentives which is rapidly increasing the efficiency of their industrial production.

"3. They have no unemployment.

"4. They have no strikes.

"5. They have a deep appreciation of pure and applied science and have placed a high priority on it.

"6. They have already planned a far more extensive program in science than any contemplated by us."

These conclusions are based on Mr. Langmuir's own observations of scientific work in Russia, as they existed shortly after the surrender of Germany.

MARTIN SEYT
Vice-President and Expediter
Plumbine Smelting Co.

Electroplates for a Stop-Off When Nitriding

PONTIAC, MICH.

To the Readers of METAL PROGRESS:

A study of nitride stop-offs, conducted at the N. A. Woodworth Co., supplements information reported by Messrs. Sternberger and Fahy in *Metal Progress.* Several kinds of plating were studied, including tin, copper, brass, bronze, chromium, and nickel. The steel tested was A.M.S.

6470 (nitralloy 135, modified); samples were nitrided 65 hr. at 975° F. in a Leeds & Northrup "homo-nitride" furnace.

It was found that the *kind* of plate was relatively unimportant as long as it was properly applied, smooth and non-porous. Due to the difference of throwing power of the various plating baths, the shape of the sample and the presence of holes and contours governed the *amount* of plate necessary for protection against nitriding. When these facts were established it was decided to concentrate our efforts on easily applied copper, brass and bronze plates.

It was soon discovered there was a relationship between the surface finish and the thickness of plate necessary to afford complete protection against nitriding. Lapped surfaces needed a very thin plate — a flash of copper, brass, or bronze being sufficient. A thickness of 0.0002 in. protected polished samples. A minimum of 0.0007 in. of copper, brass, or bronze was required to protect samples having a ground or turned surface of 35 micro-inches or more. Increasing the amount of plate would not protect a rough torn surface, due to the nodular plate built up on the crest of the tear; the current differential between the crest and the bottom of the tear made it impossible to deposit the required 0.0007 in. in the recessed area. To obtain this minimum in a depression or tool mark it may be necessary to plate to a thickness of 0.0015 in. or more on the surface or o.d.

The porosity or uneven distribution of the plate was the deciding factor and not the amount of metal deposited on the surface. A rough turned surface that was not torn was adequately protected by a minimum of 0.0007 in. in the bottom of the tool marks. Sandblasting of a *torn* surface before plating helped to reduce but did not eliminate porosity of the deposited plate. Contrary to Sternberger and Fahy's findings, sandblasting of a rough surface that was *not torn* improved the plate distribution, and a minimum plate of 0.0007 in. in the bottom of the tool marks was found to be sufficient.

In the plating of brass and bronze, the ratio of copper to zinc and copper to tin is of primary importance. With tin or zinc contents over 15% porosity occurred and protection could not be guaranteed. Complete protection was afforded by maintaining the baths so that the plate analyzed between 10 and 15%, with the preferred percentage about 12% of zinc or tin, as the case may be.

(Measurements were made by Magne-gage and by microscopic examination of polished and etched samples. For the latter, silver or chromium was plated over the sample to keep the softer plate from smearing in polishing.)

Extending the investigation to determine the effect of surface finish on nitriding it was found that burnishing or glazing of the surface by grinding with a loaded or improperly dressed wheel, or turning with dull tools, had a definite effect on the hardness, depth, or quality of the nitrided case. Burnishing by tumbling produced the same results. On some samples the case depth was retarded as much as 0.012 in., and hardnesses as low as Rockwell 86 (15N scale) were recorded. Elimination of the fret metal by snag grinding on a rough wheel, or by 5 to 7 min. in a phosphate bath, allowed the samples to nitride properly. As is to be expected, the phosphate bath exerted a stripping action on the brass, bronze, and copper. Tin plate was not appreciably affected by the bath, and the phosphate coating is a distinct aid in the prevention of tin creep.

This investigation of the relationship between casehardening and cold work was extended to carburizing, using S.A.E. 4615 and 3312 steels. It was found that the same general conditions existed as in nitriding — a surface burnished by a dull broach being especially difficult to carburize. Case depths were slightly below normal and carbon concentrations were found to be 20 to 40 points low, with a consequent lower surface hardness.

Clarence J. Miller
Formerly Chief Metallurgist
N. A. Woodworth Co.

Safety Standards for Radiologists

New York City

To the Readers of Metal Progress:

The commonly used guide for safe practices in X-ray work has been the National Bureau of Standards' handbook entitled "X-Ray Protection", first issued in 1931 and revised in 1936. A new edition was issued during the war by a group of competent radiologists headed by George Singer of the Bureau, and was designed to include provisions for installations of 2,000,000-volt and even more powerful equipment, as well as modifications of the old rules suggested by various combinations of operating factors and the wide variety of protective materials and methods of construction.

Many of the provisions in this wartime code were new, and the American Standards Association (which sponsored it) will welcome any suggestions from practicing roentgenologists as to how the code should be modified, extended, or improved to be better adapted for the needs of peacetime industry.

P. G. Agnew
Secretary, American Standards Asso.

BETTER SURE THAN SORRY

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Personals

Russell J. Barreca ⊕ has been appointed ceramic engineer and metallographer at the Pfaudler Co., Rochester, N. Y., having terminated his connection with the S.K.F. Industries, Inc.

After 3½ years in the U. S. Army, John J. Fitzgibbon ⊕ has resumed his association with the Whitehead Metal Products Co., Inc., New York City, as foundry engineer.

E. E. Hussey ⊕, formerly with the division of standardizing at Radio Corp. of America, has joined the Raytheon Mfg. Co. at Waltham, Mass., as standards engineer.

William C. Rion, Jr., ⊕ has been transferred by the Chrysler Corp. from metallurgist at the engineering division to materials and standards engineer at the Airtemp division in Dayton, Ohio.

Robert E. Atwater ⊕ is now production engineer in the copper mill of the East Pittsburgh plant of Westinghouse Electric Corp.

Robert L. Heath ⊕, manager of the St. Louis office of the Climax Molybdenum Co., has been put in charge of the company's metallurgical service to the railroads. His headquarters will be in Chicago.

Shadburn Marshall ⊕, formerly in the research department of the Remington Arms Co., is now development engineer of the Carnegie-Illinois Steel Corp., Pittsburgh.

E. C. Miller ⊕ has returned to his civilian duties as assistant professor of metallurgical engineering at Purdue University, Lafayette, Ind., following his separation from the U. S. Navy.

Fred G. Brune ⊕ has returned to the lamp division of the Electric Auto-Lite Co., Cincinnati, after 4½ years with the Army Ordnance and U. S. Navy.

R. W. S. Freeman ⊕ is now associated with H. Barnard Stamp & Stencil Co., Ltd., Hamilton, Ontario, Canada, in the capacity of assistant to the general manager.

Richard M. Wagner ⊕, formerly with Whittet-Higgins Co., Providence, R. I., is now production engineer for Allied Plastics Co., Los Angeles.

Paul Guy Maganus ⊕, recently released from Chemical Warfare Service, is presently employed as technical supervisor, U. S. Rubber Co., in Detroit.

Milton J. Winogrond ⊕ will represent U.N.R.R.A. in China as iron and steel plant engineer in regard to the industrial rehabilitation program.

After being released from the U. S. Army, Alden G. Hacker ⊕ has returned to his position at the McQuay-Norris Mfg. Co., Connersville, Ind.

C. George Segeler ⊕ has been appointed chairman of the Non-Ferrous Metals Committee of the American Gas Association.

The Certificate of Commendation of the U. S. Ordnance Dept. was awarded to Wayne L. Cockrell ⊕ for outstanding work in connection with his duties at the Detroit Ordnance District.

Yap Chu-Phay ⊕ is now in the United States gathering technical information for the reconstruction of Chinese industry. He is connected with the National Resources Commission of China.

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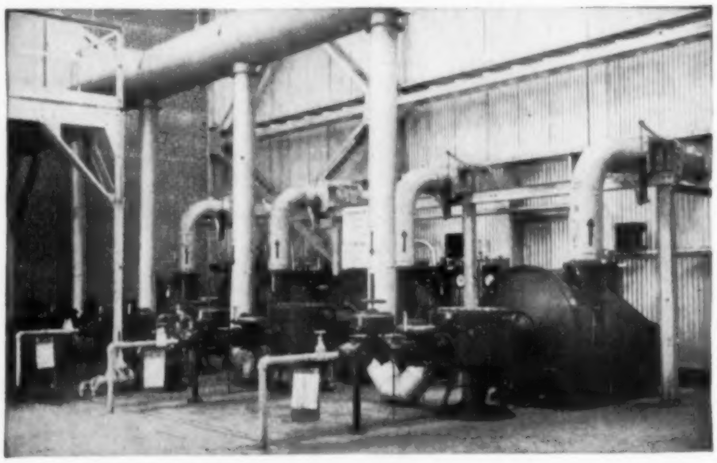

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Personals

Reynolds Metals Co. announces the appointment of R. P. Kytle, Jr., ⊕ as manager of planning at the Reynolds Alloys Co. with headquarters at Listerhill, Ala.

Donald L. Colwell ⊕, recently returned from Japan, where he was on special assignment with the U. S. Bombing Survey, Army Air Forces, is now associated with the National Smelting Co., Cleveland.

Awarded the Alumni Award Medallion of the Alumni Association of Stevens Institute of Technology: Robert C. Stanley ⊕, chairman of Stevens' Board of Trustees and president of the International Nickel Co. of Canada, Ltd.

Francis G. Jenkins ⊕ has become a member of the purchasing department of the Kodak Park Works, Eastman Kodak Co., Rochester, following his relief from duty as chief of the procurement division at the Watertown Arsenal.

Floyd B. Allen ⊕ now has a new position as methods engineer at Remington-Rand, Elmira, N. Y.

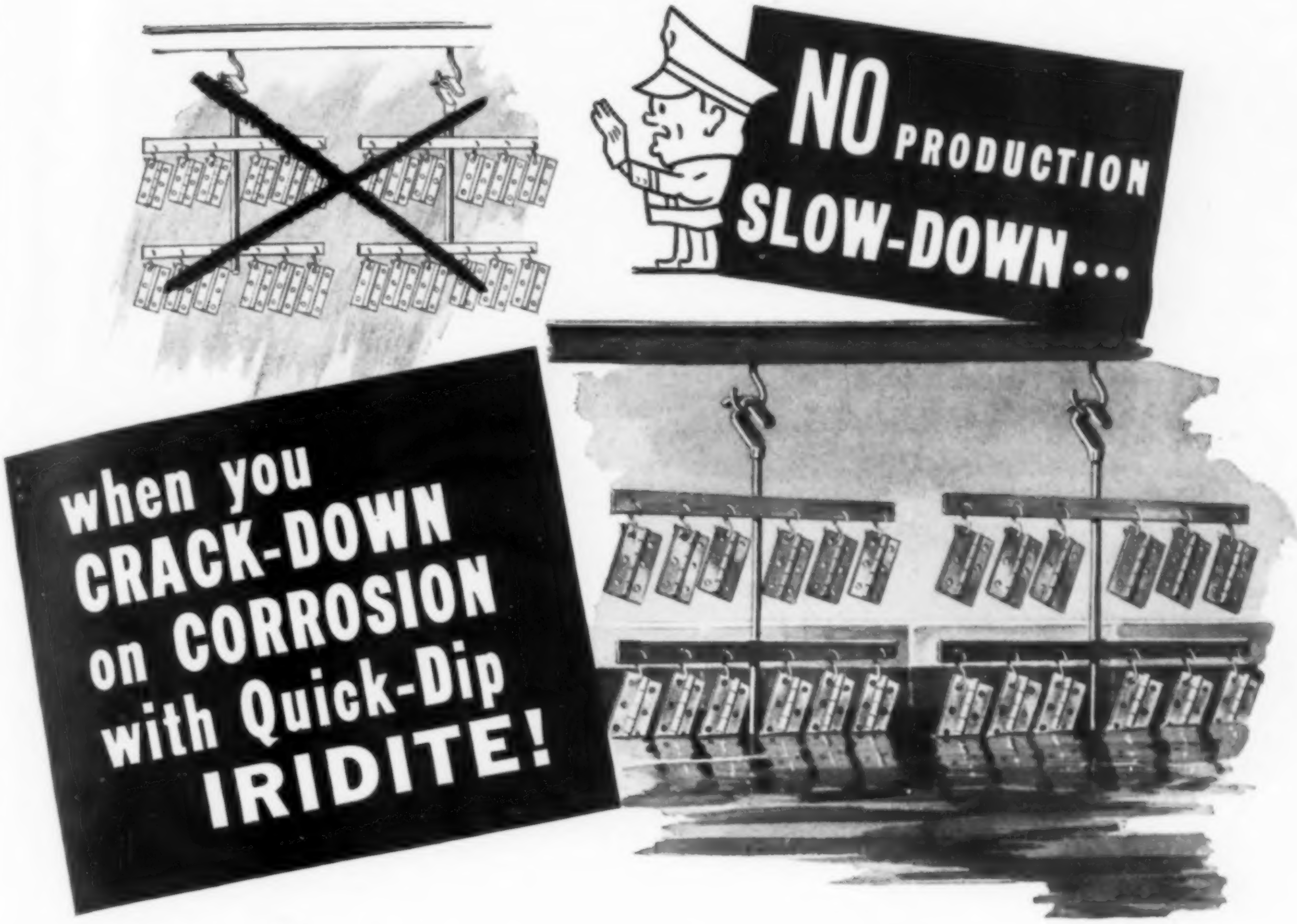
R. W. Burton ⊕, former assistant to the manager of the Jones & Laughlin Steel Corp.'s Detroit warehouse, has been appointed manager of Jones & Laughlin Steel Service, Inc., New York City.

Walter A. Gammel ⊕ has recently been appointed to the War Assets Administration at Minneapolis, Minn., in the classification unit. He was formerly inspector in charge, Lansing Branch of the Detroit Ordnance District Gage Laboratory.

Mahlon E. Wood ⊕ has become associated with the engineering department of the Chevrolet Light Car Division of General Motors Corp., Detroit, having terminated his connection with the U. S. Rubber Co.

The Army Ordnance Association's Gold Membership Emblem has been awarded to Clyde Williams ⊕, director of Battelle Memorial Institute, Columbus, Ohio, for his "outstanding service to the Army Ordnance Association in broadening its field of influence among the citizens of our country for victory and national security".

The American Welding & Manufacturing Co. announces the appointment of David Crawford ⊕ as midwest district sales manager.


NO PRODUCTION SLOW-DOWN...
when you CRACK-DOWN on CORROSION with Quick-Dip IRIDITE!

IRIDITE Provides Fast, Inexpensive Method of Protecting Zinc - Cadmium - Galvanizing

IRIDITE licks corrosion in your products . . . without once slowing up your automatic production lines. Boosts your products' sales-appeal . . . and your profits, too . . . with a quick dip and a quicker dry!

HERE'S HOW: Simply immerse your products in an IRIDITE solution . . . manually or automatically . . . on racks or in bulk . . . for 15 to 60 seconds . . . at normal shop temperatures. Your products keep moving along at production line speed . . . and they are dried in a few seconds for immediate handling. Because the IRIDITE *unites chemically* with zinc or cadmium . . . it won't flake, chip or peel when the part is bent . . . won't alter dimensions on the closest tolerance part.

MANY USES! Use IRIDITE as a final protective finish . . . on zinc or cadmium plated, galvanized steel or zinc die cast products . . . to balk corrosion while adding colorful eye-appeal. Use IRIDITE as a paint base . . . for permanent adhesion. Use IRIDITE as a cost-cutter . . . by combining it with zinc plating to replace more expensive materials. It's available in olive drab, blue, green, red, bronze, black and transparent IRIDITE Bright.

REMEMBER: Where products are threatened by rust . . . IRIDITE is a *must!* Send for the free test panel offered on this page . . . and prove it yourself. Rheem Research Products, Inc., 1434 Standard Oil Bldg., St. Paul and Franklin Streets, Baltimore 2, Md.

IRIDITE TEST-imonials

"Zinc, Iridited and painted, is possibly the last word in rust prevention as measured by extensive tests in salt spray, ammonia fumes, and various other deteriorating conditions found in structural installations."

Joseph B. Clay, *President*,
Clay Equipment Corporation,
Cedar Falls, Iowa.

Want to test that statement on your home grounds? Then . . . SEND FOR OUR FREE TEST PANEL . . . half coated with IRIDITE, half unprotected. Give it the works in your lab. See the difference. Write today!

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570 Lexington Ave., New York 22; 20 E. Jackson Blvd., Chicago 4; 2411 Sichel St., Los Angeles 31. Distribution in: Waterbury; Detroit; Minneapolis; Los Angeles.

Reg. U. S. Pat. Off.

"ROCKWELL"
HARDNESS TESTER

"A Miss Is As Good As A Mile"

That is terribly true
when shooting
for accuracy
with either a gun
or a hardness tester.

WHILE a machine to do work may be still worth a lot though it is 5% off in efficiency, that is not true of a hardness tester.

Parts are tested for hardness nowadays with the idea of rejecting them if they are one or two points of hardness off from specifications. Would you want to pass pieces that should be rejected — or reject pieces that really are of proper hardness?

Experience has shown that second hand testers have generally been so badly deranged through wear, rust, neglect, bad handling or shipment as to be worse than useless unless more is spent on them for rebuilding than old types are worth. Remember that one point of hardness on the "ROCKWELL" Tester means a difference in depth of indentation of only .00008".

AN ASSOCIATE COMPANY OF
AMERICAN CHAIN & CABLE

WILSON
MECHANICAL INSTRUMENT CO., INC.
367 Concord Avenue New York 54

Personals

After 46 months with the U. S. Army, CHARLES F. LEWIS ⊕ is back in civilian life as metallurgical engineer for the Cook Heat Treating Co. of Texas at Houston.

ROGER W. DEXTER ⊕, formerly chief metallurgist of the Chicago Screw Co., has started the Dexter Metal Treating Co. in Oakland, Calif., for commercial heat treating. HARRY E. LEWIS ⊕, who was industrial heating specialist for General Electric Co., has joined this organization also.

LARS E. EKHOLM ⊕ has joined the metallurgical engineering staff of the Climax Molybdenum Co. in New York. He has been associated with the Alan Wood Steel Co. for the past 7 years.

The Shawinigan Water and Power Co. announces the appointment of A. C. HOLM ⊕ as manager of the research department with headquarters in Montreal.

FRED P. PETERS ⊕ has been appointed associate publishing director of the Reinhold Publishing Corp., New York. He will continue to act as editor of *Materials & Methods* and in addition will expand the metalworking and materials engineering fields.

COLUMBUS FLOYD ⊕, formerly metallurgist for the Chrysler Corp. in Detroit, has become metallurgist for the Babcock and Wilcox Co. in Barberton, Ohio.

P. W. BAKARIAN ⊕ and C. H. MATHEWSON ⊕ received the Institute of Metals Division Award at the annual meeting of the American Institute of Mining and Metallurgical Engineers for their paper on "Slip and Twinning in Magnesium Single Crystals at Elevated Temperatures". At the same meeting the John Fritz Medal was presented to ZAY JEFFRIES ⊕, vice-president, General Electric Co., Pittsfield, Mass., for "leadership in the solution of problems affecting the production, conservation, substitution, and the scientific appraisal of metals and alloys". The Rossiter W. Raymond Memorial Award was presented to CAPT. JOHN H. HOLLOMON ⊕, chief of the physical metallurgical section laboratories, Watertown Arsenal, Watertown, Mass. The Howe Memorial Lecture was presented by T. L. JOSEPH ⊕, professor of metallurgy at University of Minnesota.

Q:

Which X-ray Film for examination of this multiple-thickness steel casting at 200 kilovolts?

A:

Two films— Kodak No-Screen and Kodak Type A, exposed together.

The radiographer considered three possible methods of making his examination of this steel casting with its varying thicknesses.

He could make two successive exposures, one for the thin section and one for the thick sections . . . and lose time.

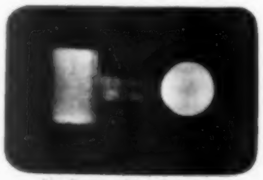
He could use one film . . . but detail would not be satisfactory in all sections.

Or—and this is what he did—he could sandwich between three lead foil screens two Kodak Industrial X-ray films . . . No-Screen, a very fast film, to pick up irregularities in the thicker portion of the casting . . . and Type A, a slower film, to hold detail and show defects in the thinner section.

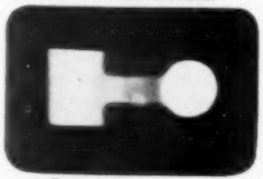
The technic he used is shown in the diagram at the right; the latitude, detail, and contrast he obtained with only *one* exposure are indicated by the two radiographs below.

TWO-FILM TECHNIC

.005-inch lead screen →
No-Screen Film →
.005-inch lead screen →
Type A Film →
.020-inch lead screen →



No-Screen Film for detail in thick sections.



Type A Film for detail in thin section.

Kodak also offers these 3 important types of industrial x-ray film

Kodak Industrial X-ray Film, Type K . . . primarily meant for gamma and x-ray radiography of heavy steel parts, or of lighter parts at low x-ray voltages where high film speed is needed.

Kodak Industrial X-ray Film, Type F . . . with calcium tungstate screens—primarily for radiography of heavy steel parts. The fastest possible radiographic procedure.

Kodak Industrial X-ray Film, Type M . . . the first choice for use in critical inspection of light alloys or—with million-volt radiography—for examination of thin steel and heavy alloy parts.

Eastman Kodak Company
X-ray Division, Rochester 4, N. Y.

Kodak

UNUSUAL CASTINGS FOR UNUSUAL SERVICE...

CONTROLLING the RANGE of guns and metals

This 27-lb cam is a component of the Sperry M7 anti-aircraft director. Serving this vital equipment called for a range control in metal characteristics: a close-grained high strength iron combining absolute freedom from distortion with good machining qualities. Castings of Absco-Meehanite met every test, were chosen for the job, and hundreds produced have proved their wear resistance as well as their rigidity . . . have been finish machined on duplicating machines to tolerances of several thousandths of an inch . . . have proved their ability to take a high polish.

ABSCO-MEEHANITE

PROPERTIES

1. Strength (Shear, Compressive, Tensile and Transverse)
2. Impact Resistance
3. Corrosion Resistance
4. Wear Resistance
5. Heat Resistance
6. Toughness
7. Rigidity
8. Machinability
9. Pressure Tightness
10. Vibration Absorption

Meehanite castings permit close control of physical properties for your specific application. Write for Meehanite Engineering Handbook

AMERICAN Brake Shoe COMPANY

BRAKE SHOE AND CASTINGS DIVISION
230 Park Ave., New York 17, N. Y.

4069

Personals

Reynolds Metals Co. announces that G. W. Birdsall ⊕ has been appointed editorial department manager of this organization. Mr. Birdsall was previously staff editorial feature writer for *Steel*.

After 4½ years in the U. S. Navy in the Office of the Coordinator of Research and Development, B. S. Old ⊕ has returned to civilian life and is with A. D. Little, Inc., chemists and engineers of Cambridge, Mass.

William Jessop & Sons, Inc., announce the appointment of Cornelius Elsasser, Jr., ⊕ as district manager of the Chicago branch office.

Paul Davis Frost ⊕ has become a member of the staff of Battelle Memorial Institute, Columbus, Ohio. Until recently he was process engineer with Curtiss-Wright Corp.

E. W. Husemann ⊕, formerly metallurgist with Copperweld Steel Co., has joined the metallurgical staff of the La Salle Steel Co., Chicago.

S. B. Knutson ⊕ has been appointed general superintendent of the Flexsteel division at the Ambridge plant of National Electric Products Corp., Pittsburgh.

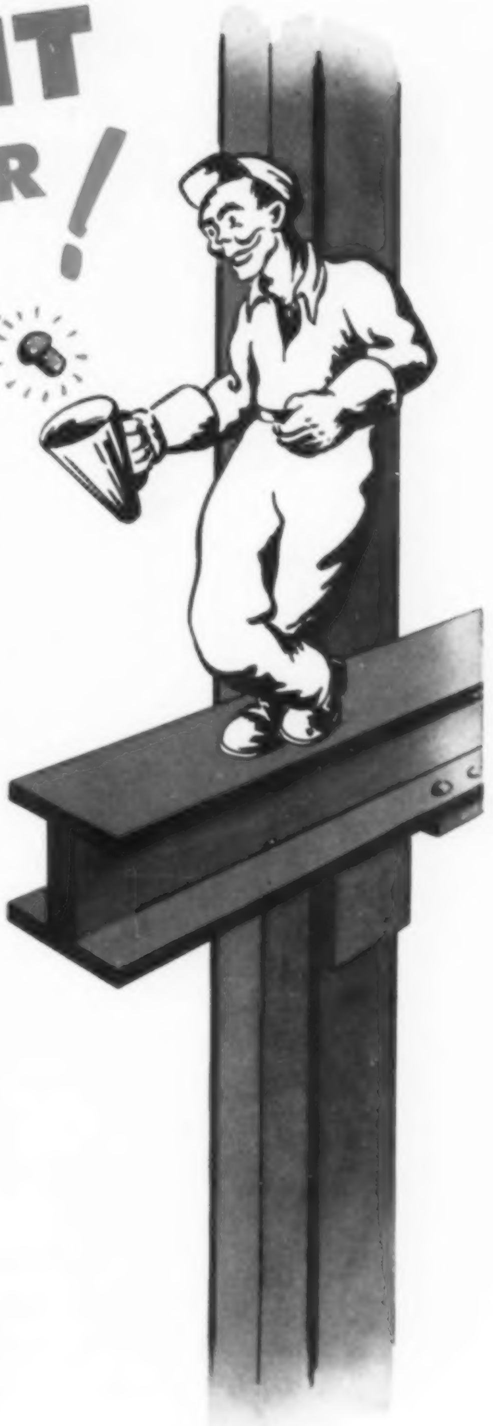
The Duraloy Co., Scottdale, Pa., announces the appointment of Max Miller ⊕ of Detroit as sales manager. Mr. Miller was formerly sales engineer of Duraloy's representative, F. B. Cornell & Associates.

Production Advisory Associates announces that Stan W. Moulding ⊕ will be district engineer of their new midwest office in Indianapolis, Ind.

Robert W. Ritchie ⊕, formerly with the Carnegie-Illinois Steel Corp., has joined Bliss & Laughlin, Inc., in the capacity of metallurgical engineer and will be located at the Harvey, Ill., plant.

Announcement comes from Paris that Pierre Chevenard has been elected to membership in the French Academy of Sciences in the division of applied sciences. Professor Chevenard has been scientific director of the Commentry-Fourchambault and Decazeville works for many years. He is well known abroad as well as in France for his many contributions to scientific methods and scientific research in the field of metallurgy.

YEARS AT IT IS THE ANSWER!



Designing an industrial heat treat furnace—like the free-and-easy performance of a structural steel worker—might appear to be a simple matter; but to attain the required degree of skill takes years of practical experience.

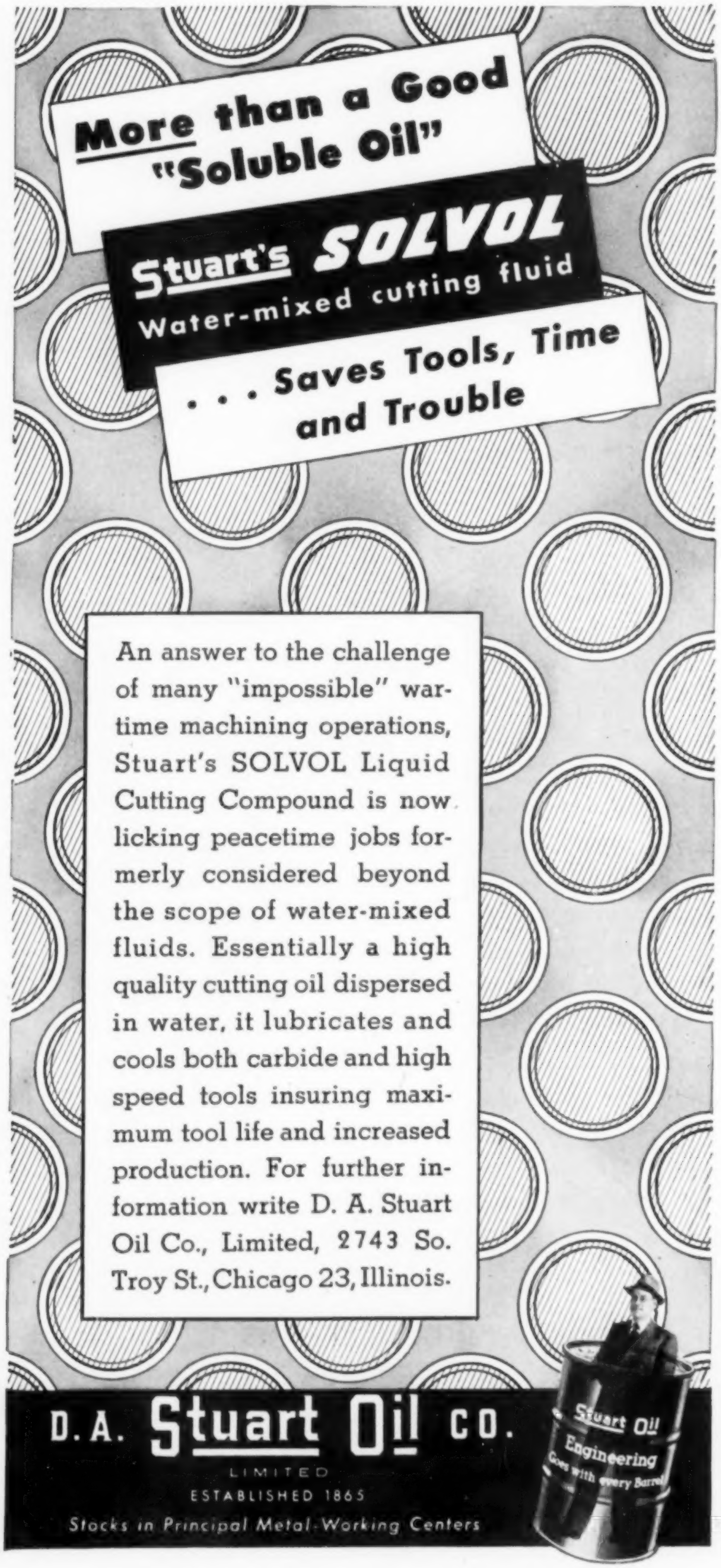
The Holcroft organization of furnace specialists offers exactly that experience. During the past 30 years we've supplied thousands of furnaces to the mass-production industries, each designed individually for the specific job.

In Holcroft is a consulting engineering service impartial to any one fuel or type of mechanism. Thus, without bias, we select the design features which are *best suited to the particular application*—frequently devising new and better methods in the process. The result is a furnace which meets the closest requirements for volume and quality of production, with maximum operating economy.

Before you invest in furnace equipment for production work of *any* type, we invite you to discuss your problem with our staff of long-time specialists—without obligation on your part.

**ELECTRIC
AND
COMBUSTION
Furnaces
BUILT
BY
HOLCROFT & COMPANY
DETROIT-MICH.
SINCE 1916—THE BEST IN
DESIGN AND CONSTRUCTION**

Holcroft AND COMPANY
6545 EPWORTH BLVD. • DETROIT 10, MICHIGAN
CHICAGO 3: C. H. MARTIN, 1017 PEOPLES GAS BLDG. • HOUSTON 1: R. E. McARDLE, 5724 NAVIGATION BLVD.
CANADA: WALKER METAL PRODUCTS, LTD., WALKERVILLE, ONTARIO



More than a Good "Soluble Oil"

Stuart's SOLVOL

Water-mixed cutting fluid

. . . Saves Tools, Time and Trouble

An answer to the challenge of many "impossible" wartime machining operations, Stuart's SOLVOL Liquid Cutting Compound is now licking peacetime jobs formerly considered beyond the scope of water-mixed fluids. Essentially a high quality cutting oil dispersed in water, it lubricates and cools both carbide and high speed tools insuring maximum tool life and increased production. For further information write D. A. Stuart Oil Co., Limited, 2743 So. Troy St., Chicago 23, Illinois.

D. A. Stuart Oil Co.

LIMITED

ESTABLISHED 1865

Stocks in Principal Metal-Working Centers

Personals

Lester R. Walker ⊕ has been promoted to chief metallurgist of the Edgar Thomson works of Carnegie-Illinois Steel Corp.

Howard H. Wilder ⊕ has been appointed chief metallurgist of the foundry division of the Eaton Manufacturing Co., Detroit. He was previously research metallurgist at the Wilson Foundry Machine Co.

The International Nickel Company, Inc., announces that Gordon S. Farnham ⊕, A. S. Tuttle ⊕, and H. J. Butterill ⊕ will staff the newly established development and research section in Toronto, Canada. Mr. Farnham has been chief metallurgist, Department of Mines, Ottawa, until his appointment to this new position. A. S. Tuttle was formerly associated with Canadian Westinghouse and with the Metals Control in Ottawa, and Major Butterill was chief design officer, British Ministry of Supply.

Charles E. Hanson ⊕ is now eastern representative for Plastic Metals Division of National Radiator Co. with offices in New York City. He was formerly associated with Metals Refining Division of Glidden Co.

Thomas F. O'Brien ⊕ has recently been appointed metallurgist on the staff of the Kali Manufacturing Co., Philadelphia.

Vincent R. Kelly ⊕ has been appointed sales engineer for the Indiana territory by the Udylite Corp. of Detroit. Mr. Kelly was previously in charge of heat treating and plating for Curtiss-Wright Corp.

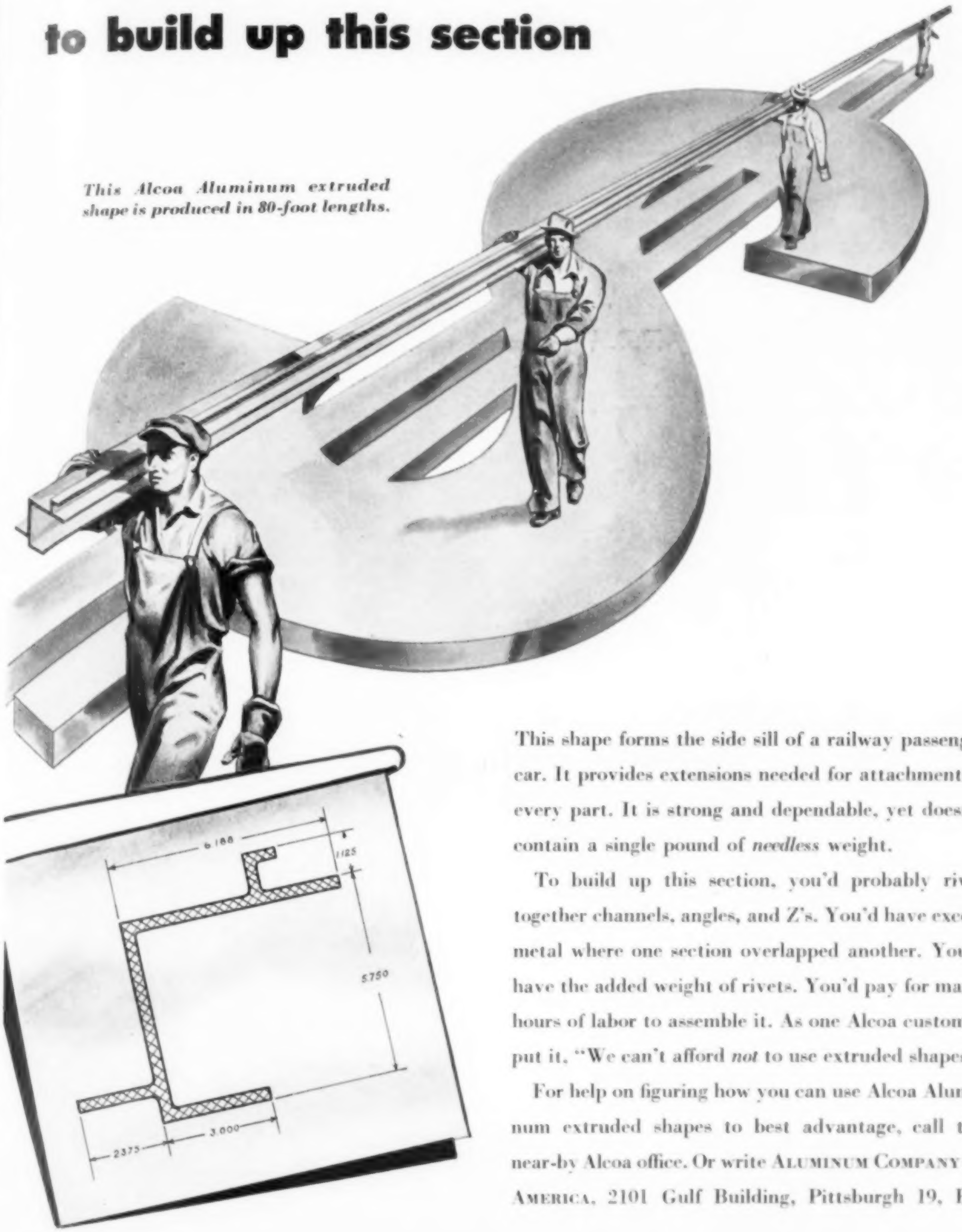
W. F. Goetz ⊕ has been put in charge of the Pittsburgh office and warehouse of A. Milne & Co.

W. R. DeGowin ⊕ has been appointed general manager of the Detroit Testing Laboratory, Detroit, after 19 years of service with this organization.

Norman E. Woldman ⊕ has resigned from the Eclipse-Pioneer division of Bendix Aviation Corp. to establish his own company, the Woldman Engineering Laboratories, Newark, N. J.

Robert E. Ward ⊕, previously assistant chief metallurgical engineer, has been promoted to chief metallurgical engineer of the Eclipse-Pioneer Division, Bendix Aviation Corp., Teterboro, N. J.

Figure what it would cost you to build up this section



This Alcoa Aluminum extruded shape is produced in 80-foot lengths.

This shape forms the side sill of a railway passenger car. It provides extensions needed for attachment of every part. It is strong and dependable, yet doesn't contain a single pound of *needless* weight.

To build up this section, you'd probably rivet together channels, angles, and Z's. You'd have excess metal where one section overlapped another. You'd have the added weight of rivets. You'd pay for many hours of labor to assemble it. As one Alcoa customer put it, "We can't afford *not* to use extruded shapes."

For help on figuring how you can use Alcoa Aluminum extruded shapes to best advantage, call the near-by Alcoa office. Or write ALUMINUM COMPANY OF AMERICA, 2101 Gulf Building, Pittsburgh 19, Pa.

ALCOA FIRST IN **ALUMINUM**

Preweld Cleaning of Aluminum*

THE PREWELD CLEANING solution for aluminum alloys developed at the welding laboratory of Rensselaer Polytechnic Institute was submitted to a six-month factory test on actual production at Armstrong Cork Co. The solution consists of dilute hydrofluosilicic acid to which a trace of a wetting agent is added. The optimum conditions were determined by daily contact resistance measurements and weldability of production pieces.

Recommended procedure for 24S-T alclad was: *(a)* immerse in alkaline degreaser for 5 min.; *(b)* rinse for at least 5 min. in air-agitated hot water; *(c)* immerse in 1.18% by weight hydrofluosilicic acid solution with 0.1% wetting agent (Nacconal NR) for 10 to 12 min. at room temperature for all gages from 0.020 to 0.125 in.; *(d)* rinse for about 5 min. in air-agitated cold water; *(e)* dry in air. Although immersion times between 6 and 9 min. were satisfactory for average conditions, the 10 to 12-min. immersion permitted satisfactory cleaning of unusual stock and did not increase the contact resistance of the average material. It was also found that the lower the initial contact resistance, the less difficulty there was with increased resistance upon continued exposure to factory atmospheres.

A rapid titration test was developed to determine the concentration of acid present in the solution. Regular additions of 27 to 30% acid were required to maintain the proper concentration (1.13 to 1.23%) in the solution. These additions were necessitated largely by losses from evaporation and seepage through the wooden tank. Daily contact resistance measurements to insure solution effectiveness and weekly concentration measurements to determine the additions were recommended for regular production control. Except for accidents, the normal life of a solution maintained by regular acid additions was at least four to six weeks. Some difficulties were encountered. A heavy black smut was formed when a fresh solution was used but it ceased to be formed after a short time. Badly stained surfaces required wire brushing to reduce the surfaces to a weldable condition.

The advantages of this cleaning solution are low cost of the raw materials, simple compounding by the user, room temperature operation, no spontaneous deterioration of the solution effectiveness, simple air drying without staining, and an increase in the number of welds between tip cleanings of 100% over the number possible with other cleaners.

The 1.18% solution was not satisfactory for 61S-W, in which the contact resistance values were high and erratic. However, concentrations from 0.50 to 0.75% by weight were found suitable in laboratory tests for both 61S-W and 24S-T alclad. Solution temperature should be over 65° F., but preferably under 80° F. Several months' use of this solution showed it to be more consistent and reliable as a preweld cleaner for the 24S-T than the 1.18% solution.

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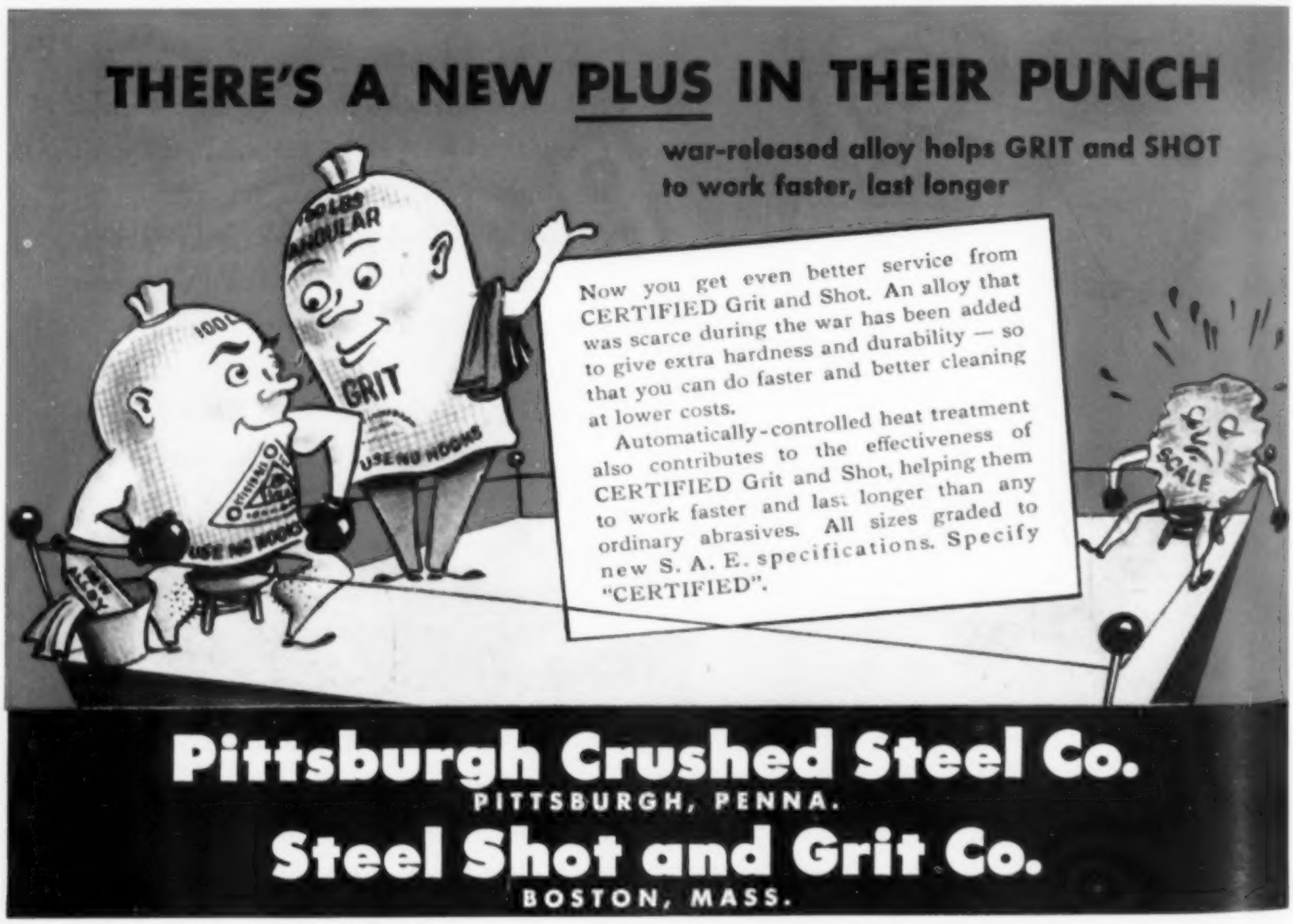
*Abstracted from "Factory Preweld Cleaning of 24S-T Alclad and 61S-W Aluminum Alloys With Hydrofluosilicic Acid Solution", by G. W. Scott, Jr., R. V. Ingram and A. A. Burr. *Welding Journal, Welding Research Supplement*, Sept. 1944, p. 443s-453s.



THERE'S A NEW PLUS IN THEIR PUNCH

war-released alloy helps GRIT and SHOT to work faster, last longer

Now you get even better service from CERTIFIED Grit and Shot. An alloy that was scarce during the war has been added to give extra hardness and durability — so that you can do faster and better cleaning at lower costs.

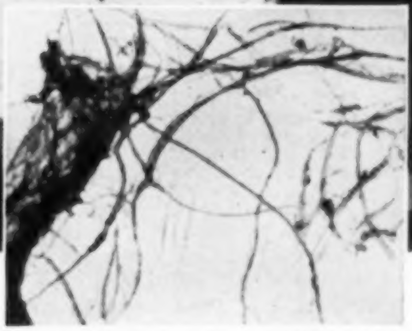
Automatically-controlled heat treatment also contributes to the effectiveness of CERTIFIED Grit and Shot, helping them to work faster and last longer than any ordinary abrasives. All sizes graded to new S. A. E. specifications. Specify "CERTIFIED".

Pittsburgh Crushed Steel Co.
PITTSBURGH, PENNA.
Steel Shot and Grit Co.
BOSTON, MASS.

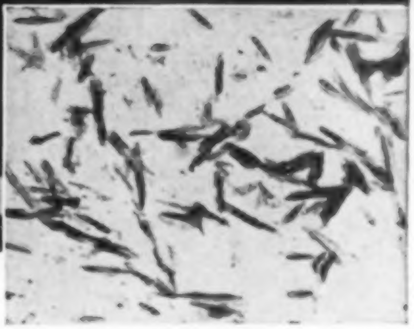
HOW CALCO USES THE RCA ELECTRON MICROSCOPE TO IMPROVE PRODUCT QUALITY

Pictures courtesy of "Textile World"

RCA Type EMC electron microscope used by Calco to speed textile and pigment research.



Disintegrated Cotton Fiber X20,000—Calco uses such micrographs to study fibers and thus determine proper dyeing technique.



Vat Green Dyestuff X20,000—Crystal size and shape are important properties in determining suitable dyeing applications.

The Calco Chemical Division, of the American Cyanamid Company, uses the RCA desk-type electron microscope in developing new and improved dyes, pigments, and textile finishes. This remarkable instrument has proved invaluable in this company's research on the size and structure of particles, surfaces, and fibers.

Magnifications of 500 or 5000 times are obtained with useful photographic enlargement up to 100,000 diameters. Calco reports:

"The electron microscope is particularly well suited to the study of pigments and insoluble dyes. For maximum hiding power, tinting strength, and coloring value, the primary particle size of pigments must be well below the dimensions that can be clearly resolved by visual light.

"The electron microscope, utilizing electrons instead of light waves, has a resolving power many times that of the ordinary light microscope, and shows with great clarity the outlines of individual particles. It reveals not only the shape and surface smoothness but frequently the structure of secondary aggregates.

"The studies which have been possible with the electron microscope have contributed materially to the development of pigments with improved properties and performance."

In an impressive number and variety of industries and institutions, the RCA electron microscope is uncovering new knowledge, speeding research, and improving product quality and performance.

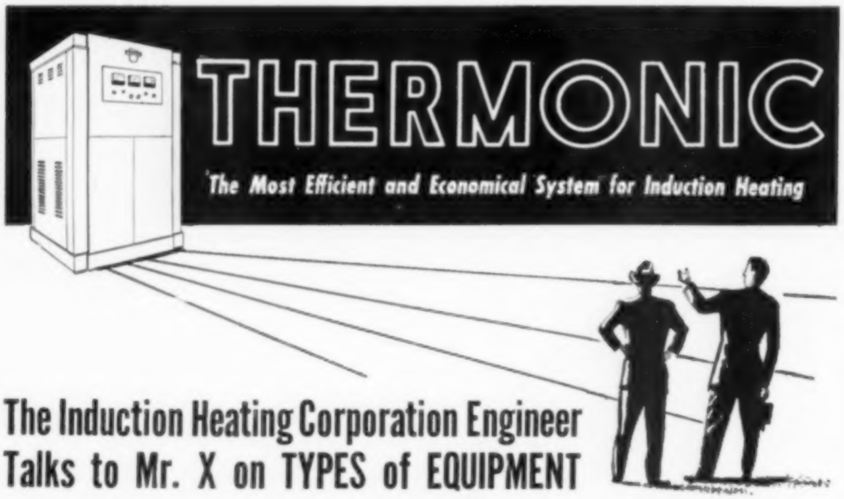
* * *

Our electron-microscope engineers will gladly help you appraise the possibilities of the desk-type or the even more versatile "universal" type RCA electron microscope in connection with your work. Write Dept. 53-D, Electron Microscope Section, RCA, Camden, New Jersey.

SCIENTIFIC INSTRUMENTS

RADIO CORPORATION of AMERICA

ENGINEERING PRODUCTS DEPARTMENT, CAMDEN, N.J.


THERMONIC
The Most Efficient and Economical System for Induction Heating

The Induction Heating Corporation Engineer Talks to Mr. X on TYPES of EQUIPMENT

MR. X . . . . From what you've told me, Mr. Engineer, it's evident that THERMONIC Induction Heating equipment offers practically no maintenance problems. But, can it do my heat-treating job any better than other types of induction-heating equipment I've seen advertised?

ENGINEER . . . . I'm glad you asked that question, Mr. X. To answer you satisfactorily, I'll have to compare the various kinds of induction-heating units and let you draw your own conclusions. For surface-heating problems, such as yours, equipment using either high or lower frequencies is applicable. Lower frequencies, ranging from 3,000 to 10,000 cycles, are produced by motor generators. Higher frequencies, ranging from 20,000 to over 1,000,000 cycles, are produced by either spark-gap converters or electronic-tube oscillators.

MR. X . . . . How can I be sure which of these three types of equipment is best suited for my job?

ENGINEER . . . . I was just coming to that. Before deciding which type of equipment to choose, you must consider three important factors: first, the size and mass of the part to be heated; secondly, just how thick a layer of the part's surface is to be heated; third, the stability of operation and range of applications of the equipment. Thus, parts of very large diameter and heavy mass lend themselves to the lower-frequency motor generators; parts of relatively small diameter or thin section, or parts requiring heat concentration in very thin surface layers are best handled by high-frequency spark-gap and vacuum-tube units. But in many cases, either high or lower frequencies can be used; thus the deciding factor in choosing equipment often becomes the relative stability of operation and scope of applications of each unit.

MR. X . . . . I don't believe the motor generator will be suitable for my particular heat-treating jobs. I need only a relatively thin hardened layer on the surface of the parts which I manufacture.

ENGINEER . . . . I agree with you completely, Mr. X. The two types of equipment for you to consider are the spark-gap and vacuum-tube units, which utilize high frequencies. The spark-gap converter produces high frequencies up to 250,000 cycles and is, therefore, capable of heat treating efficiently parts of small diameter, such as yours Mr X. But its series of spark gaps require frequent checking and adjustment in order to maintain the steady frequency necessary for operating efficiency.

MR. X . . . . How about the vacuum-tube unit? Has it any advantages over the spark-gap set?

ENGINEER . . . . I'll let you decide that for yourself. Of the three types of induction-heating units, the vacuum-tube unit, which is available in frequencies ranging from 100,000 to over 1,000,000 cycles, is the only truly electronic generator. The THERMONIC electronic-tube unit utilizes a frequency of 375,000 cycles, which has been found suitable for a wide variety of heat-treating and brazing operations. Incidentally, electronic tubes are used exclusively in radio broadcasting as fixed-frequency generators.

MR. X . . . . But how can the fixed frequency of a vacuum-tube benefit me specifically?

ENGINEER . . . . That's simple, Mr. X. In all precise heating jobs like yours, the stabilized frequency and power output of vacuum-tube oscillators insures consistently uniform results from day to day, with unskilled help. Such high-quality production cannot be obtained by spark-gap units, where the frequency and power output fluctuate.

MR. X . . . . Then it would seem that the vacuum-tube unit is best fitted for my particular heat-treating operations.

ENGINEER . . . . You're absolutely right. Remember, vacuum tubes proved their superiority over other methods of frequency conversion a long time ago in radio broadcasting. This superiority is as outstanding as are the electronic-tube, radio-broadcasting stations of today over the motor-generator and spark-gap stations of twenty-five years ago. And in line with this, the Induction Heating Corporation, which built the first commercial electronic-tube, induction-heating unit, is proud of its contributions in the development of this modern, electronic type of equipment.

THERMONIC
INDUCTION HEATING CORP. NEW YORK

INDUCTION HEATING CORPORATION
389 LAFAYETTE ST. · NEW YORK 3, N. Y.
Largest Producers of Electronic Heat Treating Equipment for Forging
Brazing · Melting · Hardening · Annealing

Grain Refinement of Magnesium Alloys*

FINE GRAIN is necessary in magnesium alloys to avoid hot cracks and to obtain the best mechanical properties. There is some evidence that a coarse grain size causes microporosity and makes necessary a longer holding time at the solution temperature. However, if grains are too fine, they lead to the formation of very large grains during heat treatment.

The present commercial method of grain refinement involves superheating of the molten alloy. This process has certain disadvantages. Its cost is substantial because of the time, fuel and labor required as well as the considerable amount of equipment and short pot life. The temperature-time cycle must be carefully controlled for uniform results and the superheating must be carried out on relatively small batches. The increased iron absorption decreases the corrosion resistance of the alloys. Cooling rate is important; slow cooling gives coarser grains, and it was shown definitely that a coarse grain size accentuates the occurrence of microporosity.

A new method of producing grain refinement in magnesium alloy melts containing aluminum involves the addition to the melt of a carbonaceous material such as a carbon-containing gas, graphite, aluminum carbide, or vapor from an organic chloride. A carbonaceous gas such as natural gas was found to be as effective as superheating. However, the method was somewhat awkward and time-consuming and regassed the melt if the gas used was not dry. The temperature at the time of treatment should not be less than 1350° F. About a 6-min. treatment was ample for a 120-lb. melt. Attempts to degas and refine the grain simultaneously by using a mixture of natural gas and chlorine were not successful.

Carbonaceous solids (graphite, aluminum carbide, silicon carbide and high carbon ferromanganese) were usually effective, although the results were somewhat erratic because of the difficulties of obtaining proper solution of the carbon. A remarkable grain refinement was obtained by *(Cont. on page 800)*

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*Abstracted from "Grain Refinement of Magnesium Casting Alloys", by James A. Davis, L. W. Eastwood and James DeHaven. *American Foundryman*, Aug. 1945, p. 34-44.

Light Metals give your product new selling features!

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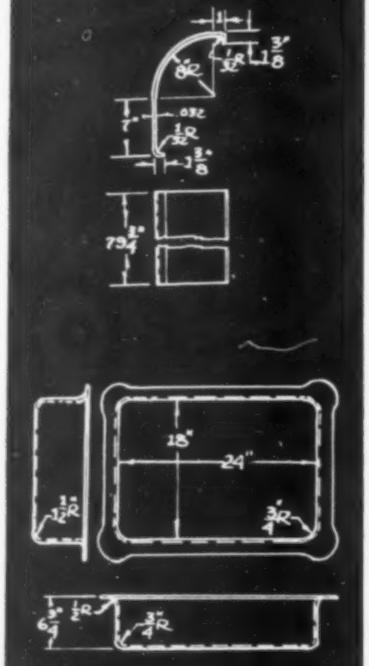

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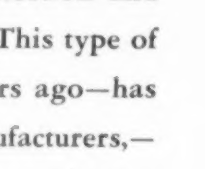

Grain Refinement

(From p. 798) melting in a silicon carbide crucible. Such crucibles, possibly with a carbon lining for large melts, may have some practical possibilities.

Carbon hexachloride and tetrachloride were very reliable, gave good grain refinement and also provided some cleaning and degassing action. A mixture produced by bubbling chlorine through liquid tetrachloride degassed, cleaned and refined the grain size in a single operation. This procedure has been very successful on melts up to 120 lb. and it should be applicable to larger melts. A temperature of 1350 to 1450° F. was suitable. The grain size of castings from such melts was increased by holding near 1250° F. This adverse effect gradually decreased with increasing temperatures until only a very slight coarsening occurred after several hours at 1400° F. The grain-refining effect lost by holding at low temperatures was restored by simply reheating to 1400 to 1450° F. Each melt should be individually treated, since the carry-over cannot be relied upon.

The general theory of grain refinement by carbon inoculation is consistent with most or all of the observed experimental facts. Aluminum carbide (Al_4C_3) — the active grain-refining constituent — is dissolved in the melt. When the melt is cast, the aluminum carbide is precipitated near or slightly above the liquidus line at which solidification begins, producing a cloud of nuclei around which the individual grains form. The greater the number of nuclei per unit volume of melt, the finer the grain. ◆

Biaxial Fatigue Strength*

MOST FATIGUE TESTS have been made under uniaxial stress conditions although biaxial or triaxial stress conditions are present in most engineering applications.

Thin-walled cylindrical specimens were subjected to fluctuating internal pressures synchronized in phase with a *(Cont. on page 802)*

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*Abstracted from "The Biaxial Fatigue Strength of Low Carbon Steels", by George K. Morikawa and LeVan Griffis. *Welding Journal, Welding Research Supplement,* March 1945, p. 167s-174s.

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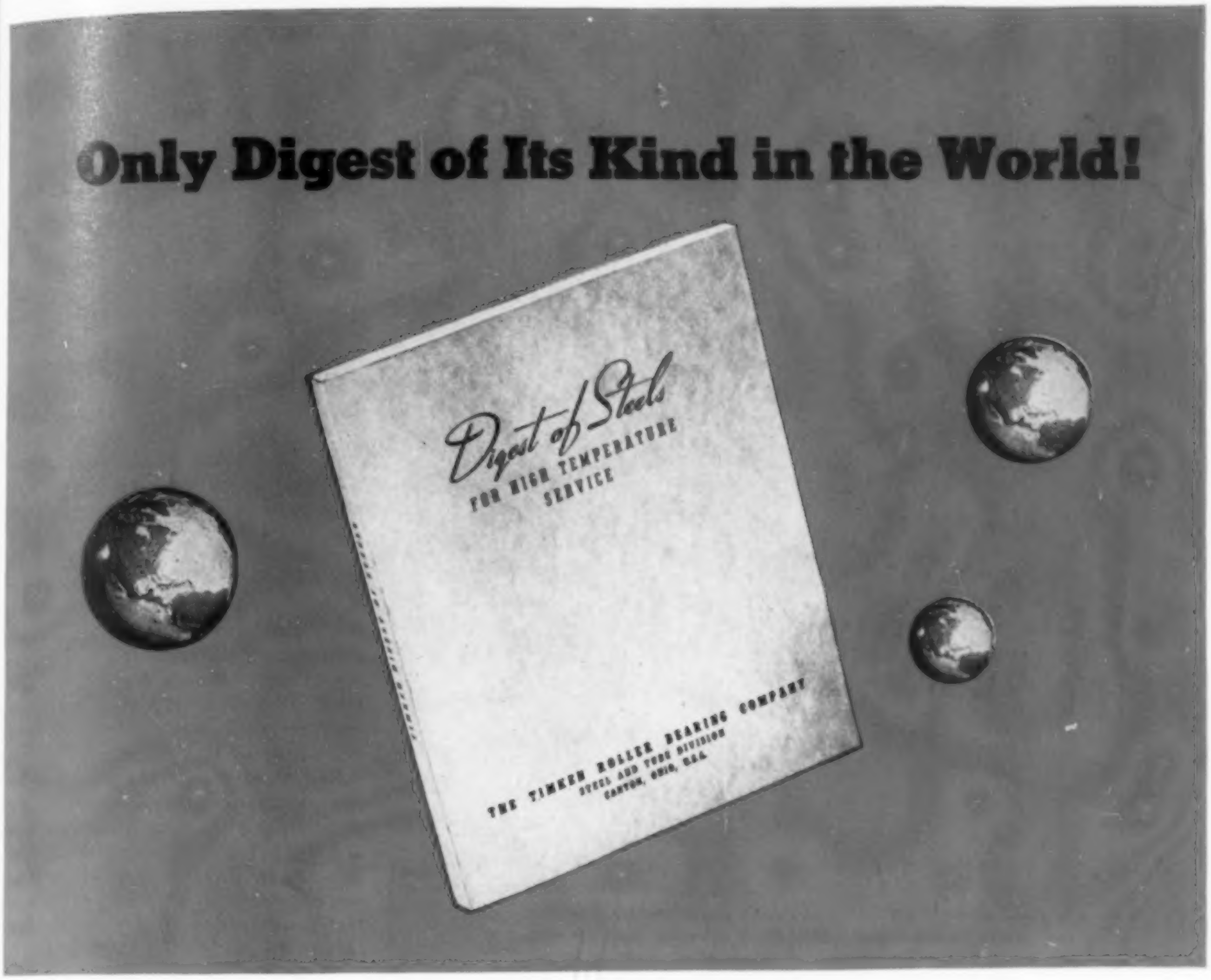
Digest of Steels
FOR HIGH TEMPERATURE SERVICE
THE TIMKEN ROLLER BEARING COMPANY
STEEL AND TUBE DIVISION
CANTON, OHIO, U.S.A.

A 215 page compilation detailing the physical properties of 23 Timken high temperature steels at elevated temperatures! Contains hundreds of photomicrographs, curves and tables essential to the user in evaluating various analyses and choosing the one best suited to meet required operating conditions.

Here between two covers is the authoritative guide to high temperature steels accepted as standard everywhere in the industry. It is published by The Timken Roller Bearing Company, acknowledged to be *the only company in the world which has the background of research to produce it.*

Results of original research by The Timken Company—as typified by this Digest—has solved many problems and made important savings for users of high temperature steels. If you have a problem, call or write The Steel and Tube Division, The Timken Roller Bearing Company, Canton 6, Ohio.

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ELECTRIC MELTING of ALUMINUM ALLOYS

No Longer a Luxury

Aluminum alloy iron-core induction melting furnaces, the first of this kind capable of continuous operation, have been developed at Ajax's Experimental Foundry (see photo upper right). Simplified cleaning methods and improved design of melting channels have resulted in increased lining life and reduced maintenance cost.

Job of melting 300 pounds per hour requires 60 kw. unit (see photo above) occupying about 4′ x 4′ x 4′ space, requiring no foundations and provided with a self-contained internally wired control cubicle, including potentiometer type temperature controller. Operating cost from 40 to 70 cents per hour, with maintenance items almost negligible.

Another unit of 20 kw. capacity is finding wide acceptance as holding furnace in die casting and permanent mold work. Space required is about 3′ x 3′ x 3′, no foundations, self-contained control cubicle. Operating cost from 8 to 12 cents per hour. Metal charge of crucible is 300 pounds.

X-ray investigation carried out on metal processed in Ajax induction furnace proves that accurate (free of time lag) temperature control, typical of these furnaces, allows casting consistently at lowest and most adequate temperatures necessary for sound castings, all of which results in considerable reduction of rejects.

AJAX ENGINEERING CORPORATION
TRENTON 10, N. J.

AJAX TAMA-WYATT INDUCTION MELTING FURNACE

Associate Companies: AJAX METAL COMPANY, Non-Ferrous Ingot Metals and Alloys for Foundry Use
AJAX ELECTROTHERMIC CORP., Ajax Northrup High Frequency Induction Furnaces
AJAX ELECTRIC CO., INC., The Ajax-Hultgren Electric Salt Bath Furnace
AJAX ELECTRIC FURNACE CORP., Ajax Wyatt Induction Furnaces for Melting

Biaxial Fatigue Strength

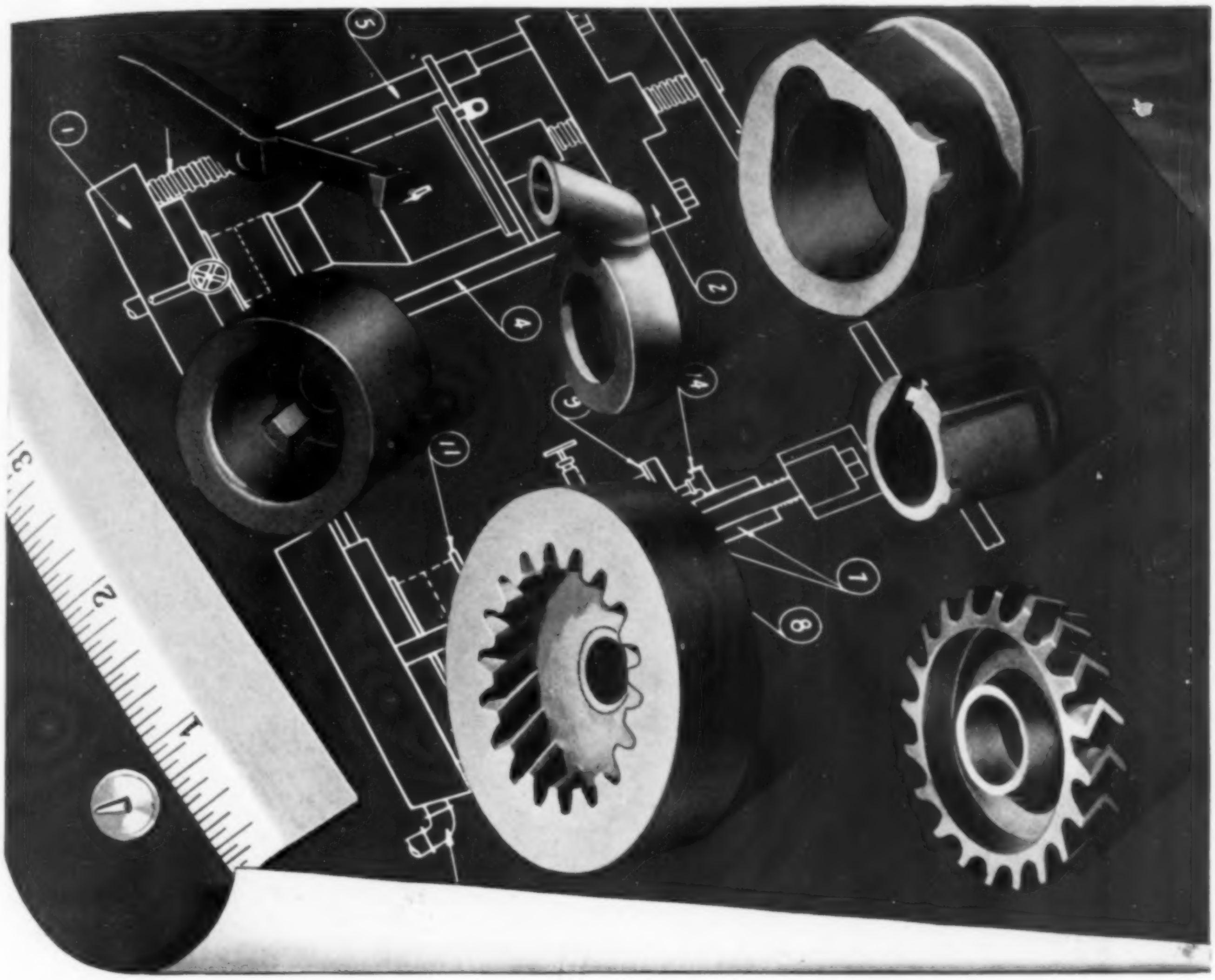
(From p. 800) fluctuating axial tension load. The ratio of biaxial principal stresses was held constant during any one test. Tests were made on silicon-killed S.A.E. 1020 in annealed, normalized, and annealed and welded condition.

For a ratio of circumferential stress to axial stress n ranging from 0 to 2, there was only a slight effect of the principal stress ratio on the endurance limit. For the test conditions used, the endurance limit was apparently of the same magnitude as the yield point. The fatigue strength of normalized steel was not appreciably different from that of the annealed steel for $n = 2$ (internal pressure only). The results did not permit establishing a theory of fatigue failure under combined stresses which would be appreciably different from the maximum stress theory for this ductile steel. The effect of plastic deformation or cold work has not been determined. The anisotropy of this rolled steel was a major factor in the occurrence of fatigue even after annealing. The endurance limit was about 15 to 20% lower when the stresses in the transverse direction were predominant. Static control tests and Izod impact tests verified this decrease in strength.

The preliminary tests on the welded specimens indicated that the fatigue strength of the welded specimens was about 15% less than that of the unwelded specimens for $n = 2$. However, this result was obtained both when the fractures occurred in the welds and in the parent metal. This weakness may be due to residual stresses or heat effects although it will be difficult to determine this in the very thin-walled specimens.

The strain distribution in the plastic range under cyclic combined stresses for all cycles up to the endurance limit showed zero strain at failure in the direction of the intermediate principal stress for $n = \frac{1}{2}$ and $n = 2$. This agreement with the results found in other work for static and creep conditions was unaffected by the anisotropy of the present steel.

The equipment used has two inherent disadvantages, namely, the extremely low speed of operation and the necessity for thin-walled specimens. It is proposed to study heavier material at higher speeds to obtain more directly useful data on welded structural elements. ◆



A Better Basic Design

MICROCAST PRECISION CASTINGS PROCESS

THROUGH MICROCAST

Forward looking design engineers in their constant effort toward product improvement have found the means to a better basic design through the MICROCAST PROCESS. With MICROCAST it is now possible to utilize the unusual properties and many advantages of a whole new range of alloys—Vitallium, Stellite, stainless and tool steels which often in the past could not be exploited because their use would entail prohibitive production costs.

MICROCASTINGS of these high melting point alloys may be quantity produced in intricate shapes and designs, with such sound structure, surface smoothness and precision tolerances that little or no machining is required.

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Consider MICROCAST in your own product plans particularly for small parts where high tensile strength or resistance to wear, corrosion and high temperatures is required. A better basic design through MICROCAST may be the means to a better product for you.

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FURNACES
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Above: End view.

Left: Top view.

Right: Side view.

Four men at the controls of six Type LFY Detroit Rocking Electric Furnaces produce 6000 lbs. of bronze per hour (a 750 lb. heat every 7 minutes) in this compact foundry layout. One man charges the furnaces, two men operate them, and one supervises. The result is maximum utilization of manpower, space and equipment—under most desirable working conditions—for the production of high quality castings. Send us your requirements, and our engineers will determine, without obligation, whether a similar Detroit Electric Furnace layout will provide more economical melting of ferrous or non-ferrous metals in your plant.

Right: Electrodes are controlled hydraulically from stationary pedestals which contain all automatic electrode and rocking controls for regulating melting speed, power input, and other melting factors. Transformers are installed beneath furnaces.

Left: Charging trucks are loaded on the scales in the metal room and elevated to the charging platform which separates two rows of three Detroit Electric Furnaces, installed back to back. Rear charging is accomplished quickly, eliminating congestion in front of furnaces.

DETROIT ELECTRIC FURNACE DIVISION
KUHLMAN ELECTRIC COMPANY • BAY CITY, MICHIGAN

Boron Addition Agents*

EFFECTS OF BORON addition agents on cleanliness, grain size and hardenability were determined on 119 laboratory heats with up to 2.47% carbon, up to 1.73% manganese and up to 1.76% silicon. Sixteen types of boron-bearing addition agents were used, namely, Silvaz, Silcaz, Bortam, Carbortam, Borosil, titanium boride, ferroboron, manganese-boron, boron carbide, fused boron trioxide glass, Pyrobor (dehydrated borax), chromium-boron, nickel-boron and three experimental ferro-alloys containing boron and zirconium.

The quinalizarin colorimetric method of analysis for boron was found to be simple, rapid and accurate. The total boron content is the sum of the boron present in an insoluble and in a soluble portion of the sample. Both the soluble and the insoluble boron increased linearly with total boron; the relative amount of soluble boron decreased with increasing carbon, silicon and manganese.

None of the addition agents was detrimental to the cleanliness. Likewise, no coarsening of the grain size due to the boron was observed. Hot shortness was found only when the boron exceeded 0.017%.

Regardless of the method of addition to the steel, boron had a specific effect on the hardenability. The Grossmann factor increased with the boron content to a maximum value of 1.58 at 0.003% total boron. The multiplying factor decreased with further increases in the boron content. It would appear that the distinction between soluble and insoluble boron is due to chemical procedure and is not reflected in the hardenability results. Except for Silvaz and Silcaz, the elements other than boron present in the addition agents did not increase the hardenability measurably. The high silicon contents in some of the addition agents is also likely to affect the final silicon analysis of the melt.

The method of addition of the boron was not critical if the bath had been properly killed and cleared of slag prior to the introduction of the boron-bearing addition agent. In most cases, the addition agent was merely dropped on the slag-free *(Cont. on p. 806)*

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*Abstracted from "Effects of Boron in Steel", by R. B. Corbett and A. J. Williams. *Bureau of Mines Report of Investigations* 3816, June 1945, 21 p.

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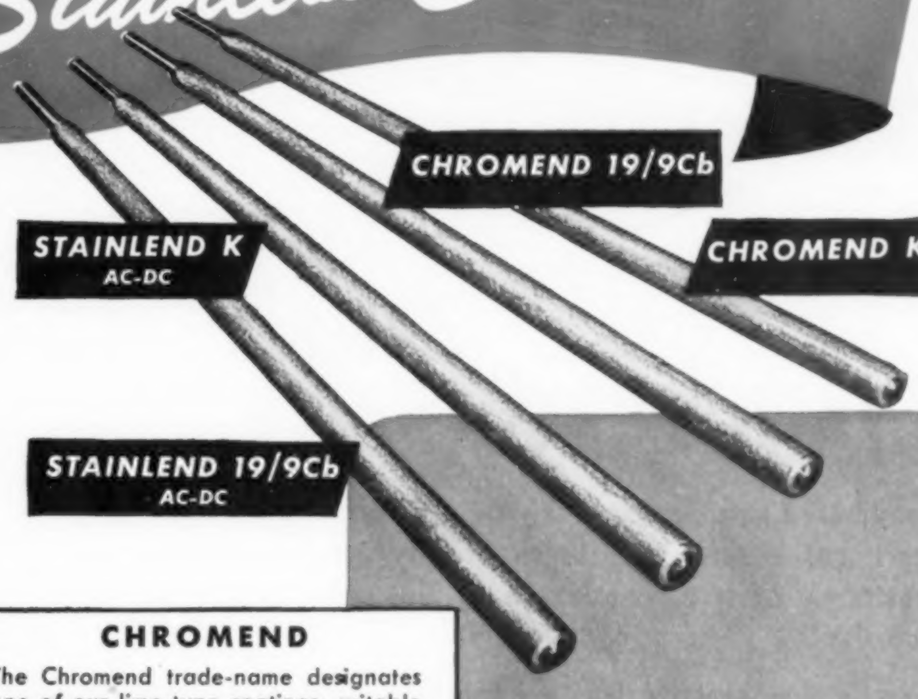

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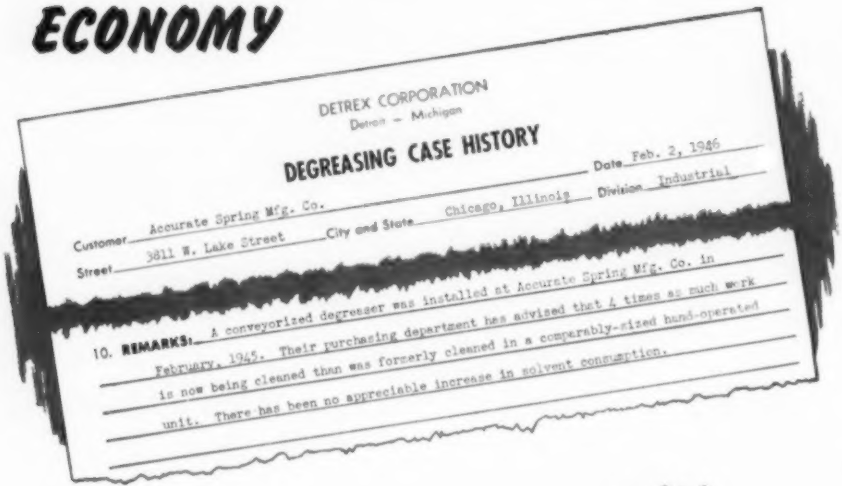
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Detroit – Michigan

DEGREASING CASE HISTORY

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Customer Accurate Spring Mfg. Co.
Division Industrial
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City and State Chicago, Illinois

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DETREX *Corporation*
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E-115

Boron Addition Agents

(Cont. from p. 804) metal surface. Boron could be added satisfactorily by means of any of the addition agents used. The range of efficiencies for each type of addition agent was too great to warrant classifying the agents according to the boron recovery; this variation was probably due to the small amount added and to variable chemical composition of the additions.

Silvaz, Silcaz and titanium boride appeared to dissolve with difficulty. Titanium boride also caused excessive pipe. Boron carbide tended to give an explosive reaction on addition as well as a poor ingot surface. Ferroboron was an excellent source of boron. Refuting previous information, dehydrated borax and fused boron trioxide glass, both cheap, were found to be suitable addition agents.

The effect of small amounts of boron on hardenability is comparable to that of much larger additions of the more common alloying elements. The hardenability increase caused by 0.003% boron is equal to that resulting from 0.87% silicon, 0.79% nickel, 0.27% chromium, 0.12% manganese or 0.23% molybdenum. Generally speaking, boron additions are most valuable in medium carbon steels which are to be liquid quenched and tempered at a low temperature.

Hot Workability of Steel*

A QUANTITATIVE hot workability test has been devised based on the rapid twisting of heated bars. The number of turns to failure is determined for a series of temperatures. It is possible to specify the optimum hot working temperature and to predict whether or not a heat of steel will survive various hot working processes by the correlation of these results with mill operations. Some steels, particularly stainless steels, show a marked difference in hot workability from heat to heat. Therefore, critical steels must be tested individually to find the proper hot working temperature.

(Continued on page 808)

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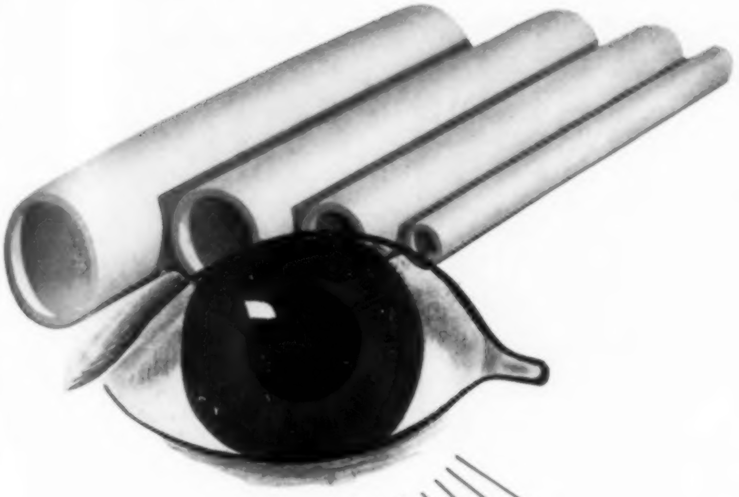
*Abstracted from "The Effect of Various Elements on the Hot Workability of Steel", by Harry K. Ihrig. American Institute of Mining and Metallurgical Engineers Tech. Pub. No. 1932, *Metals Technology*, Oct. 1945, 29 p.



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Hot Workability of Steel

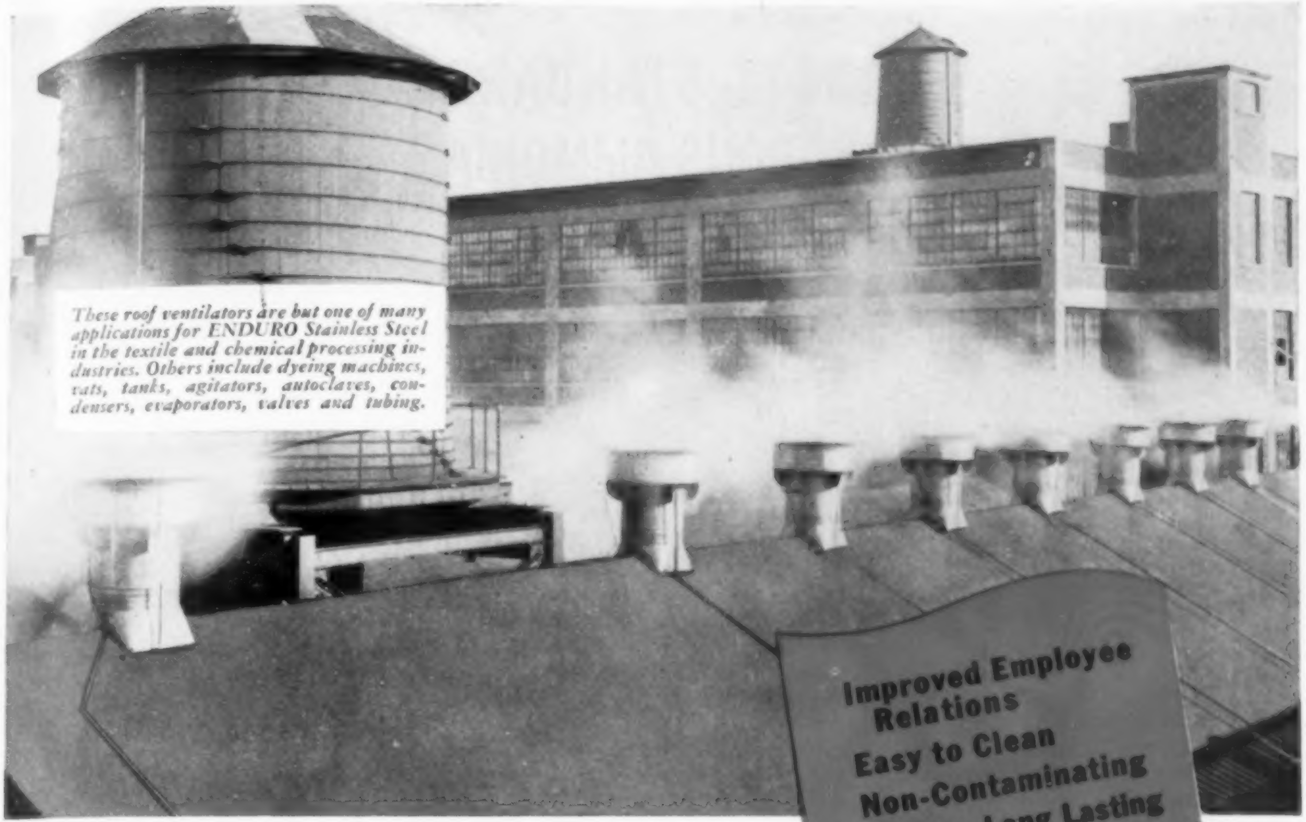
(Continued from page 806)

Deoxidation practice does not materially affect the hot workability of low carbon steels. In killed carbon steels, the maximum ductility rises with increasing temperatures for carbon contents under 0.25%. Higher carbon steels have their maximum ductilities at 2350° F. or lower. As the carbon content increases, the ductility falls off rapidly with increasing temperature. The hot workability of chromium stainless steels decreases with increasing carbon content. The lower carbon Cr-Ni stainless steels have poorer workability than the ones with 0.08% carbon because of their delta ferrite.

Most alloying elements tend to decrease the hot workability. Sulphur and selenium have a very deleterious effect on the workability of carbon steels. The addition of sulphur as sodium sulphite gives only slightly better hot workability than corresponding amounts added as elementary sulphur. Lead and tin have a strikingly similar effect to sulphur for equal additions. High chromium appears to neutralize the red shortness tendency induced by both sulphur and lead. Silicon up to 0.2% has little effect on low carbon steels but over 1% decreases the hot workability. Silicon contents over 0.5% markedly decrease the hot workability of austenitic chromium-nickel steels. Molybdenum reduces the hot workability of carbon as well as austenitic chromium-nickel steels. However, it has little effect on 5% chromium steels. The hot workability of chromium steels is decreased by nitrogen, while columbium markedly depresses the hot workability of 5% chromium-molybdenum and austenitic chromium-nickel steels.

Oxygen, phosphorus, cobalt, vanadium and titanium individually have little if any effect. Chromium seems to have an adverse effect up to about 9%. Above this amount, it noticeably increases the hot workability. Other than high chromium contents, manganese and nickel are the only alloying elements that improve the hot workability of steels. Manganese overcomes some of the detrimental effect of sulphur. Nickel and manganese tend to be additive in their effect on austenitic stainless steels. However, 13% manganese steel has much lower workability than ferritic steels.

(Continued on page 810)


These roof ventilators are but one of many applications for ENDURO Stainless Steel in the textile and chemical processing industries. Others include dyeing machines, vats, tanks, agitators, autoclaves, condensers, evaporators, valves and tubing.

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Hot Workability of Steel

(Continued from page 808)

The room temperature austenitic steels have considerably poorer hot workability than the room temperature ferritic steels. Austenitic steels with ferrite-forming elements or with insufficient amounts of austenite-forming elements tend to have duplex structures which have the poorest workability of all.

Graphitization*

WELD TESTS were made to determine the effect of a few variations in the type of steel, original treatment, speed of welding and stress relieving on the graphitization after 2000 hr. at 975° F. and after 3000 hr. (2000 hr. at 975 followed by 1000 hr. at 1050° F.). A narrow and a wide weld bead were laid down on opposite faces of the following samples: *(a)* fine-grained 0.5% molybdenum steel (from the pipe that originally failed by graphitization); *(b)* coarse-grained 0.5% molybdenum steel; and *(c)* fine-grained S.A.E. 1020. All specimens were normalized from 1650° F. before welding while steel *a* was also tested after a normalize from 2000° F. After welding, half the samples were stress relieved for 4 hr. at 1300° F.

After the 3000-hr. reheating, no graphitization was found in any of the samples except in the un-stress relieved steel *a* as normalized from 1650° F. Localized graphitization occurred in this sample in the heat affected zone of the weld laid down at a relatively rapid rate. Less graphite, more widely dispersed, was found in the heat affected zone of the wide weld bead; this graphite had formed more slowly than that near to the narrow weld bead. This specimen also showed occasional bits of graphite in the unaffected base metal. It was noteworthy that stress relief after welding successfully prevented localized graphitization.

End-quenched samples were also tested to determine the effect of initial structure on the graphitization. The steels used were: (1) coarse-grained 0.5%

(Cont. on page 812)

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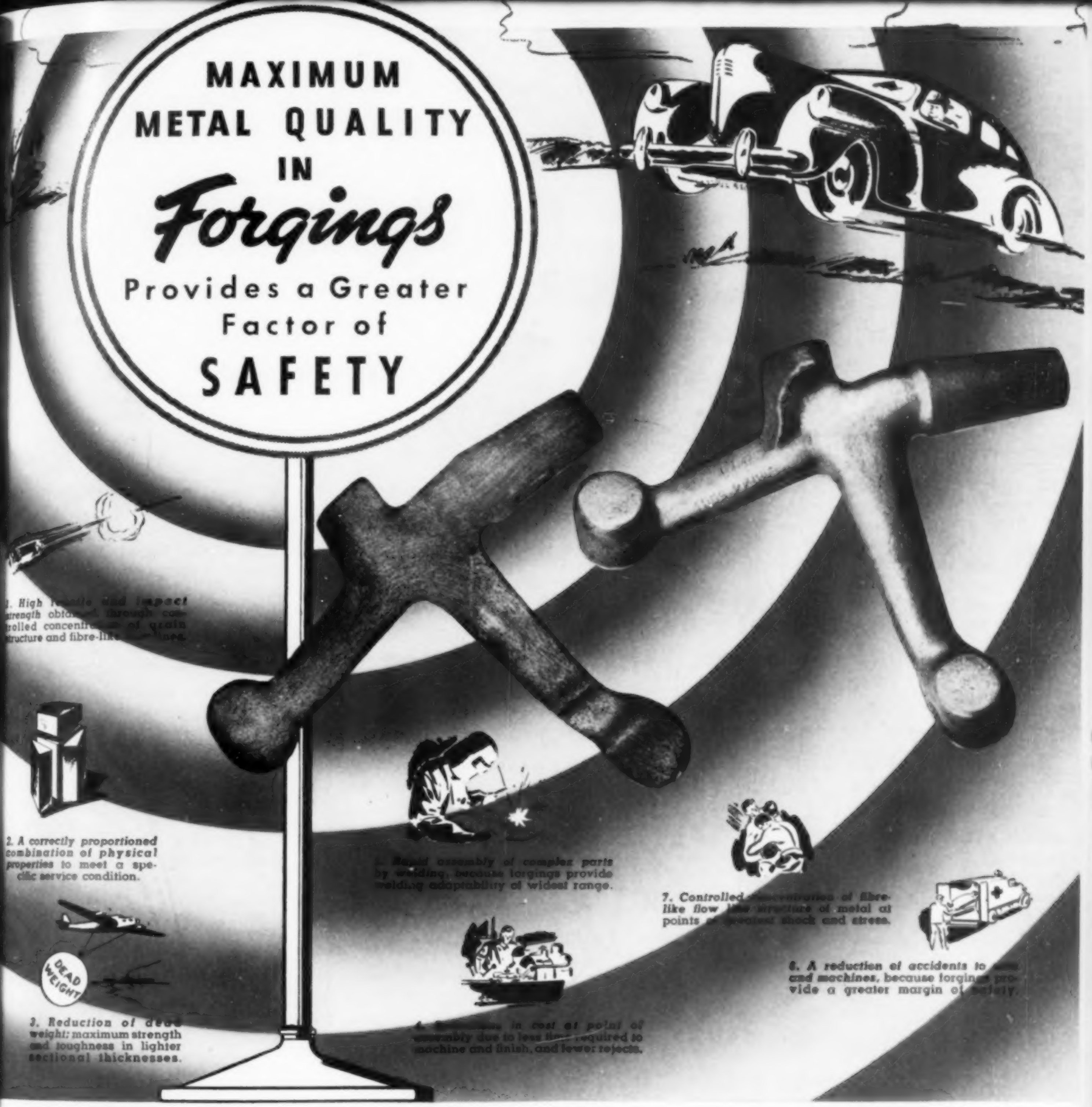
*Abstracted from "Graphitization of Welded and of End-Quenched Carbon and Molybdenum Steels", by G. V. Smith and S. H. Brambir. *Welding Journal Welding Research Supplement*, March 1945, p. 153s-156s.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2. A correctly proportioned combination of physical properties to meet a specific service condition.
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7. Controlled concentration of fibre-like flow line structure of metal at points of greatest shock and stress.
6. A reduction of accidents to men and machines, because forgings provide a greater margin of safety.

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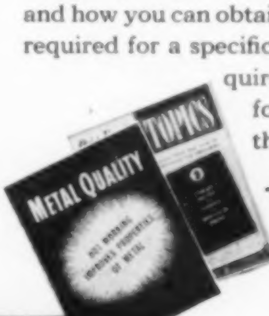
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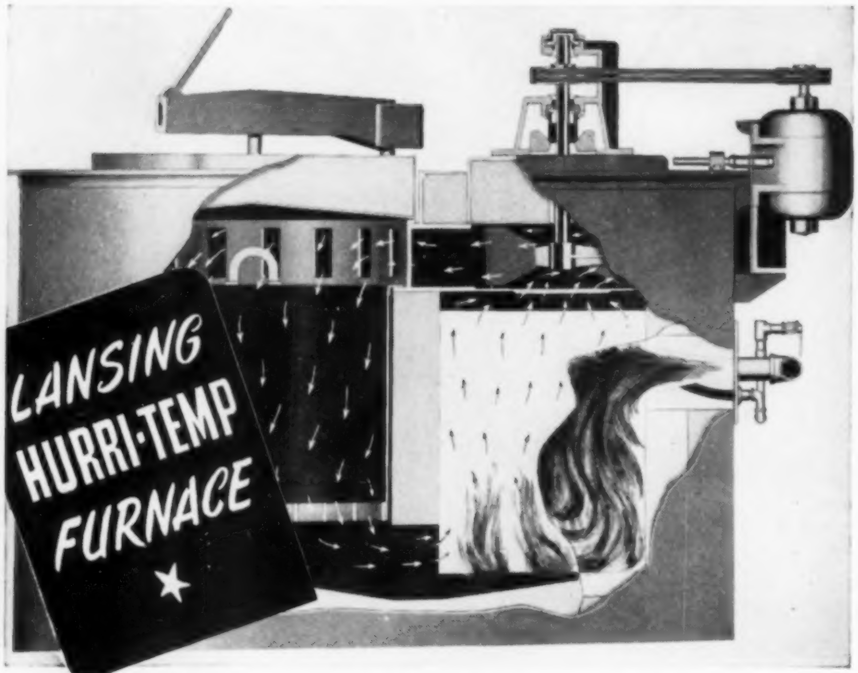

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Graphitization

(From p. 810) molybdenum steel; (2) fine-grained 0.5% molybdenum steel; (3) fine-grained S.A.E. 1015; (4) fine-grained S.A.E. 1045; (5) fine-grained S.A.E. 1080; (6) Bain metal; and (7) 5% chromium, 0.5% molybdenum steel. Steels 1 to 5 were end-quenched from 1650° F. Immediately after quenching, two of the samples of steel 2 were heated for 3 hr. at 1200° F. and 24 hr. at 1300° F., respectively. Steels 6 and 7 were normalized from 1700° F. and tempered 4 hr. at 1300° F. The reheating schedule for all samples was the same as for the weld tests.

None of the samples of steels 1, 2, 6, and 7 showed any graphite after 3000 hr. All the samples of steels 3, 4, and 5 graphitized. The martensitic regions had many small nodules of graphite; the transition area had larger segregated nodules, while the pearlitic portion had fewer but still larger isolated nodules of graphite. Further tests indicated that the graphite in carbon steel was stable at 1300° F. and continued to grow, whereas the graphite in a sample of 0.5% Mo steel had previously been shown to revert to carbide at 1300° F.

The discrepancies between the results of the weld tests and the end-quenched tests prove that there is yet a great deal that is not understood about the phenomena of graphitization.

X-Ray Movies in the Foundry*

A TECHNIQUE for taking X-ray motion pictures of the filling of a mold with molten metal has been developed. It promises innumerable advantages and possibilities to the foundryman. An X-ray tube is mounted behind the mold while a fluorescent screen is placed in front. The resultant image of the mold on the screen is photographed with a motion picture camera.

At first a 200-kv., 40-milliamp. X-ray set was used but it was found that a 140-kv., 8-milliamp. set not only gave a brighter picture but also had a very small target area so the linear definition *(To p. 814)*

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*Abstracted from "Ciné Radiography", by S. L. Fry. *Metal Industry*, July 6, 1945, p. 2-28.

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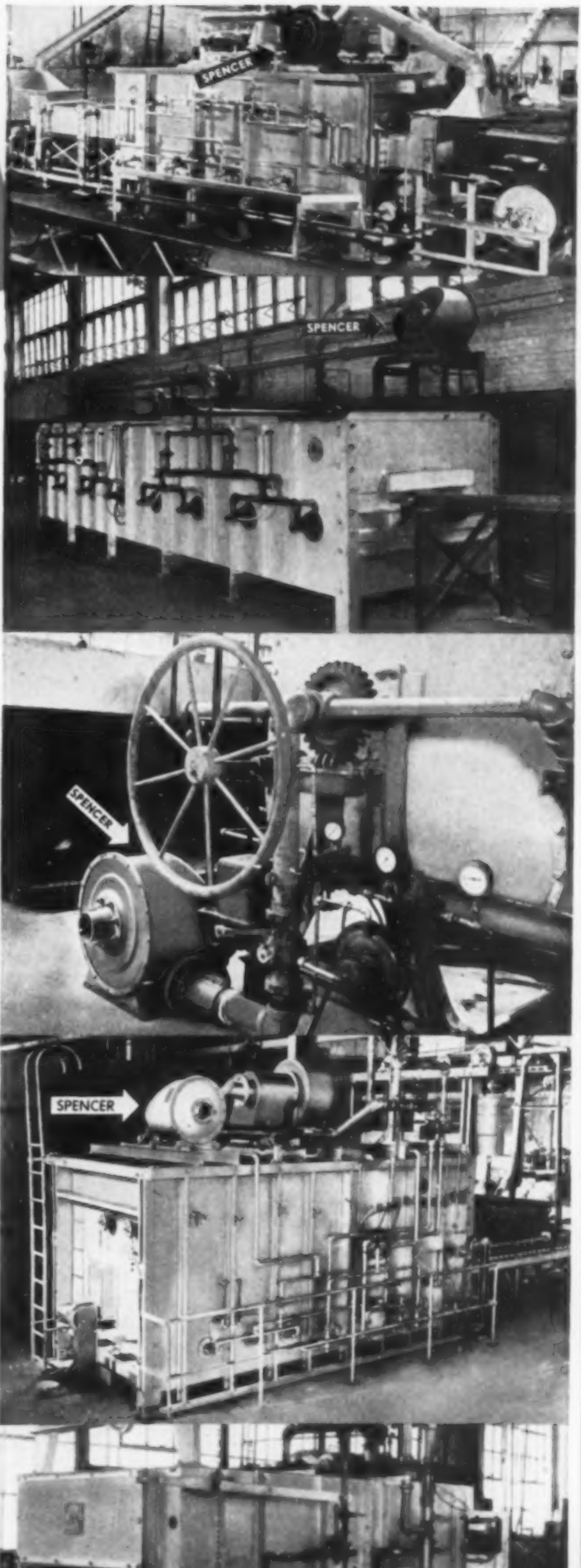

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X-Ray Movies

(Cont. from p. 812) was appreciably finer and the contrast was improved. The mold size was limited to an area of 11x9 in. with a maximum metal thickness of ⅝ in. and a maximum sand thickness of 1⅞ in. Unsupported core sand layers could be used for the mold. The first pictures of molten lead had a high contrast which was greatly decreased in the subsequent work on aluminum. However, the contrast was increased by adjustments in the camera, film and developer. A 35-mm. camera fitted with a 5-cm. f/1.5 Sonnar lens, driven by a motor, was finally chosen. A fast contrasty film was adopted. The photographic procedure was fairly standard.

Some of the films were run on a motion picture projector but it was felt that the results should be printed rather than shown as a motion picture. Moreover, if the pictures were taken in slow motion, the fluctuation of the screen intensity caused by the incomplete rectification of the a.c. for the X-ray set was disturbing. In general, the camera was run at a number of frames per second until the casting was filled; then individual exposures were made at intervals of 10 sec. This practice was quite satisfactory since the growth of shrinkage cavities was slow. The formation of gas bubbles was too rapid to enable its recording in detail.

The preliminary results showed the effect of the speed of pouring, of the use of a swan neck runner, of venting, of horizontal pouring and of the growth of shrinkage cavities. A fast pour caused severe turbulence; the large surface area subject to oxidation was clearly shown. Even with slow pouring, the jet had sufficient power to force through the accumulating metal. With a swan neck runner, differences in the rate of pouring caused differences only before the metal passed through the swan neck. Air retarded the flow of metal up the mold unless the mold was vented; this difference was more marked for the faster pours. The vented casting also showed more gas bubbles. With horizontal pouring, the last cavity was at the end of the runner jet; therefore it was demonstrated quite conclusively that the hottest metal was at the main point of delivery in the casting and not at the immediate junction between gate and casting. ◆

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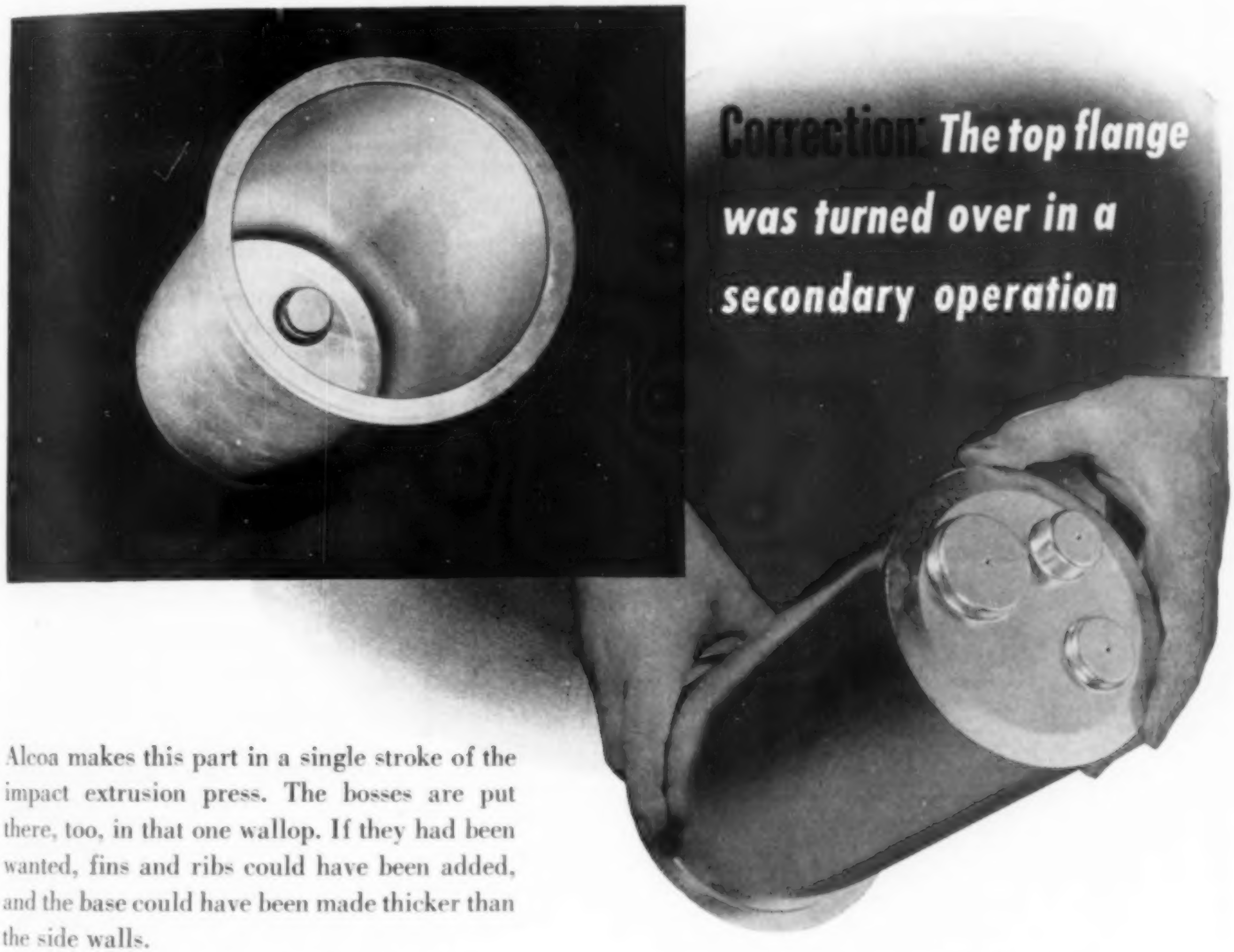

Correction: *The top flange was turned over in a secondary operation*

Alcoa makes this part in a single stroke of the impact extrusion press. The bosses are put there, too, in that one wallop. If they had been wanted, fins and ribs could have been added, and the base could have been made thicker than the side walls.

Remember how you once figured on three or four drawing operations to make a part like this? Punches, dies, press time, and intermediate heat treatments all ran your costs up *high*. And even then, you'd have to find some way of tacking those bosses on the bottom.

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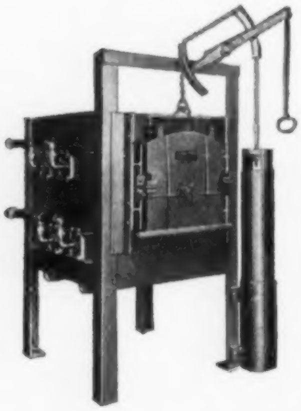

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THE STANDARD extrusion machine is built in two general styles — a double-action machine generally used to extrude tubing, and a single-action machine usually used in the extrusion of rod. Machines with a pressure capacity over 1200 tons are usually horizontal. The few vertical machines in this country manufacture tubing and have limited capacity.

In the normal extrusion of copper and brass, the actual extrusion time is 30 to 45% of the total cycle. Because of the presence of oxidized material, 15 to 25% of the billet is always discarded. The container for the hot billet is built up of three sections with the liner a hard heat resistant alloy. In ordinary brass extrusion, a dummy of somewhat smaller diameter than the container liner is commonly used between the ram and the hot billet. In the extrusion of tubes, the container must be prefilled by upsetting the billet by the main ram before the introduction of the piercing ram. The piercing mandrels are generally chromium-tungsten or chromium-tungsten-molybdenum steel with the alloy content low enough to prevent cracking when water cooled between extrusions. When high extrusion temperatures or longer times are necessary, higher alloy mandrels are used with oil cooling. The available alloy tool-steels permit using pressures over 100,000 psi. of ram pressure but they require considerable care. It is particularly necessary to preheat the tools before use and to maintain them at a suitable temperature.

There are several important differences in the extrusion of copper-base alloys as compared with aluminum and magnesium. In the extrusion of light alloys, tool temperatures must be maintained within narrow limits. The rate of extrusion is also very slow compared with that used for the copper-base alloys. Many of these alloys cannot be hot worked by any other method because the tensile stresses set up in other processes produce cracking. A slow rate of extrusion keeps the temperature of the material low enough to avoid incipient melting or overheating the extruded material. *(Cont. on p. 818)*

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*Abstracted from "The Extrusion Process", by W. W. Cotter and W. R. Clark. American Institute of Mining and Metallurgical Engineers Tech. Pub. No. 1850, 11 p.

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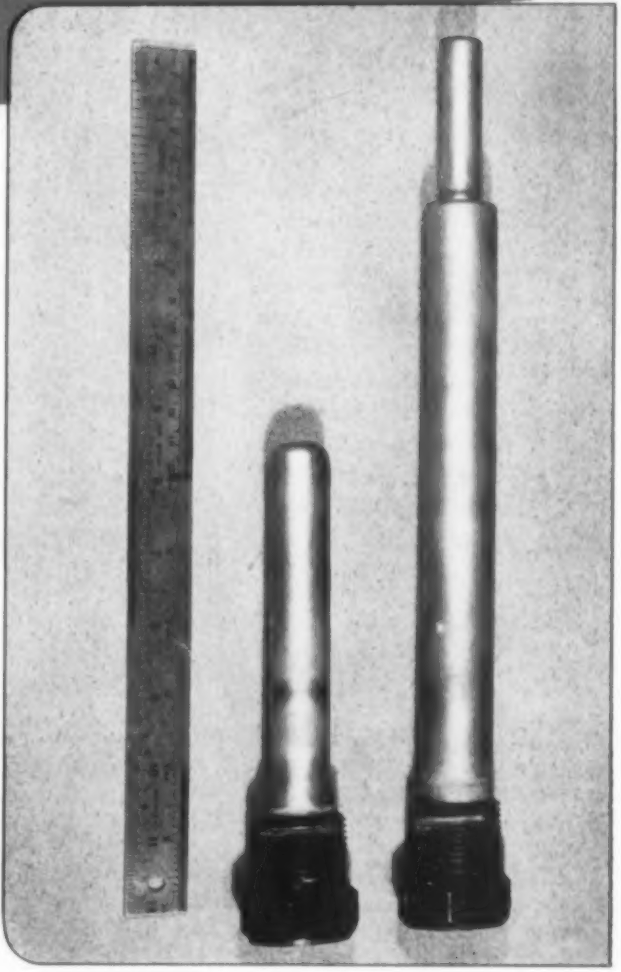

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Extrusion

(Continued from page 816)

The required pressure on the dummy varies with the alloy, speed of extrusion, temperature of the material, container, die and dummy, ratio of the dummy area to the die area and billet length. The starting pressure at normal extrusion rates is usually the maximum. The dummy pressure increases rapidly when the billet length is greater than its diameter due to the increased frictional effect of the container on the billet surface. Multiple hole extrusion is used where the amount of diameter reduction for the length-diameter ratio is too large to permit successful extrusion of a single small section. Multiple extrusion requires somewhat greater pressures than a single hole extrusion with an area equal to that of the multiple die but not in proportion to the benefits derived. Tube extrusion shows a slightly higher minimum extrusion pressure and a somewhat lower maximum than the average for rod extrusion. Extrusion speeds are considerably faster for tubing than for rods to avoid overheating the mandrel.

Even though the starting pressures are less in reversed extrusion, this process is little used since it is difficult to obtain extruded material free from surface defects caused by some of the oxidized billet surface being drawn into the die near the surface of the extruded section. The extrusion must be completed rapidly enough to prevent an excessive temperature drop in the billet. A difference of 10% in resistance may be caused by a variation of 1.5% copper in screw machine rod or by a 45° F. temperature variation in cartridge brass.

After the extrusion starts, the part of the billet being deformed by extrusion through the die is raised in temperature; this raises the temperature and reduces the resistance of the adjacent material to deformation. The approximate theoretical temperature rise thus produced may be calculated, but not all of this temperature rise is manifest in the extruded rod under ordinary extrusion speeds.

Extrusion pressures are similar to those calculated from hot hammer tests and are far greater than those found in slow hot tensile tests. It is believed that very rapid extrusion rates would require pressures approaching those shown by the hot hammer test.

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